Popular Astronomy

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Volume Fifty Concluded

Reference to the heading of this page shows the number of this issue to be 500. As each volume of this publication consists of ten numbers, this total indicates that with this issue the fiftieth volume is completed. Since each volume generally includes the issues of one calendar year, the first volume was begun before the beginning of this century. This fact together with the large number of issues conveys the impression of a continuous and an enduring institution. It is, perhaps, worthy of mention that throughout the period of fifty years no issue has failed to appear as scheduled.

One naturally expects conditions at the end of a fifty-year interval to be different from those at the beginning. However, the form, size, and general appearance of this issue are strikingly similar to those features in the first issue. In some other respects normal changes have occurred. The reproducing of illustrations and the quality of the printed page have been definitely improved, as is to be expected in consideration of the general advancement in proficiency in all mechanical arts. Also the character of the contents has changed in keeping with the changing and advancing viewpoint of the science with which the magazine is chiefly concerned.

It might seem that after fifty years a magazine, which deals principally with the sky and the "fixed" stars, would have fulfilled its purpose. But even the "fixed" stars are moving and other new information concerning the heavens is daily being brought to light. These new facts need to be presented continually in order to keep those who are interested abreast with current developments and discoveries. POPULAR ASTRONOMY hopes to continue to serve in this capacity in the future as it has tried to do in the past.

This magazine has been able to maintain an uninterrupted existence for half a century for three reasons. 1. It has had the interest and sympathetic support of professional and amateur astronomers the world over. 2. It has had the benefit of a reasonably large number of readers both in America and abroad. 3. Carleton College, the sheltering institution for about two-thirds of this period, has been willing to absorb such small deficits as occur from time to time in its budget.

POPULAR ASTRONOMY acknowledges with gratitude these sustaining elements and with this issue presents its first fifty volumes harboring the hope of continued approval.

Galileo*

By GEORGE ROSENGARTEN†

The year Galileo died Newton was born. We have heard this statement before; we today commemorate the 300th anniversary of the death of Galileo. I shall ask you to consider with me some of the outstanding events in the life of Galileo.

"I think with your friend that it has been of late too much the mode to slight the learning of the ancients."—Benjamin Franklin.

I want you to see in Galileo a young active college professor carrying on extensive research of a most fundamental nature in many branches of mathematics and science. This was in addition to his regular lectures at the University of Pisa. He had just reached his twenty-fifth year.

I want you to picture his laboratory, very different from the laboratory of 1942. Galileo was not the first to question Aristotle on the matter of science; he was the first to make an impression. I believe he had great admiration for Aristotle. In one of his dialogues, Salviati is made to say, "I am encouraged by the example of Aristotle whom I admire especially because he did not fail to discuss every subject which he thought in any degree worthy of consideration."

It is a long time from the time, when a certain young professor in the University of Pisa was observing the motion of a ball rolling down an inclined plane, to the present, when certain other professors in many parts of the world, especially in the United States, are bombarding atoms with high-speed electrically charged particles. Can you grasp that span of 300 years in space and time? During that time those scientifically minded have been traveling step by step, and we today are standing upon the shoulders of the fathers of science who have gone before.

Aristotle, Archimedes, Hipparchus, Lucretius, Ptolemy had preceded the dark ages prior to 150 A.D. Science began to step out in search of truth after 1200. Roger Bacon, Leonardo da Vinci, Copernicus, and Galileo were among the research workers of that early day. Scientific progress is made by adding a few facts to those that have been passed on from teacher to pupil, generation after generation. Galileo's own words, "But this and other facts not few in number or less worth knowing, I have succeeded in proving: and what I consider more important, there have been opened up to this vast and most excellent science, of which my work is merely the beginning, ways and means by which other minds more acute than mine will explore its remote corners."

Galileo Galilei, as he was called, was born in Pisa, Italy, on February 15, 1564. His father was a merchant who possessed more than an

*Paper read at a meeting of the Rittenhouse Astronomical Society on January 9, 1942.

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ordinary interest in music and was very much interested in the education of his son in whom he recognized unusual ability. Galileo was the eldest of three sons and three daughters. In the monastery, as a boy, Galileo studied Greek and Latin and was later sent to the University of Pisa to study medicine, at considerable sacrifice to his family. But Galen did not interest the young student after he became acquainted with the ideas of Archimedes and Aristotle. This was in 1581. In order to appreciate the work of this young man we must put ourselves back into the scientific world of the 16th Century. Innumerable fallacies existed among the most generally received opinions. The teachings of Aristotle and Ptolemy had been passed along for more than a thousand years with little advancement.

1. It was natural movement for heavy bodies to fall and light bodies to rise.
2. Heavy bodies fell faster than light bodies.
3. The earth was the center of the universe and the seven planets including the sun revolved about the earth.

Such was the teaching Galileo received at the University—ideas not sufficiently tested by experiment.

To be sure Roger Bacon, Leonardo da Vinci, and others had experimented but their work was not made known to the general public because of the antagonism with which new ideas were received in certain quarters.

Bodies had been dropped and observed to gain in speed. Stones were hurled into the air and observed to move along a certain curved path, but no careful investigation into the nature of their motion had been carried out. Before any progress in the field of physics or astronomy could be made it was necessary to conduct such experiments in motion on a much more elaborate scale. We call it the scientific method. I have followed Galileo in his discussions, his mathematical analysis, and his experimentation and he is truly the Father of Scientific Research.

What manner of research did Galileo carry on? He early became interested in the motion of a pendulum. He observed that its oscillations took place in equal times no matter what the displacement from the central position. This was a scientific fact that had not been previously added to man's knowledge of the workings of nature. Galileo later carried out many experiments on the motion of the pendulum. That observation of the swinging pendulum, I believe, was the result of the efforts of some forgotten teacher who had impressed upon the young Galileo the importance of careful observation. It is not uncommon for the pupil, receiving that which his teacher expounds, to far surpass him in intellectual development. That is scientific progress.

Galileo's dislike for the study of medicine caused him to change his course and pursue in more extensive manner the subjects of science and mathematics. Much impressed with the study of Archimedes he wrote

an essay on the hydrostatical balance as used by the ancient philosopher in detecting the composition of the golden crown. He was perhaps one of the earliest physicists to be engaged in research in industry, for he was called upon to investigate the subject of the center of gravity. These early efforts were noted by distinguished mathematicians, and at the age of twenty-five he was appointed to a lectureship at the University of Pisa in 1589. For three years he lectured to the students and carried on extensive research in the field of moving bodies, research that was necessary to pave the way for modern physics. He carried out experiments on falling bodies to verify or to disprove the teaching of Aristotle. He rolled bodies down inclined planes because of greater ease in observing the acceleration.

I hope you will stop here and repeat some of the fundamental research as conducted by Galileo. Aristotle is reported to have stated that heavy bodies fall faster than light ones. You drop two bodies of different weight, a coin and a book, and you observe they both reach the floor in the same time. Perhaps the height was too small. Galileo experimented with greater heights. Have you?

Let me read from "The Two New Sciences" of Galileo. This was not published until 1638, near the end of his most active career and contains the results of years of research into many different fields of mathematics and science. Galileo considered this work superior to everything else he had published.

These new ideas, many of them contrary to the teaching of Aristotle as he was understood almost 2000 years after his death, were in the form of a conversation carried on by three persons.

"Salviati: I greatly doubt that Aristotle ever tested by experiment whether it be true that two stones, one weighing ten times as much as the other, if allowed to fall, at the same instant, from a height of, say, 100 cubits, would so differ in speed that when the heavier had reached the ground, the other would not have fallen more than 10 cubits."

Simplicio: His language would seem to indicate that he had tried the experiment, because he says: we see the heavier; now the word see shows that he had made the experiment.

Sagredo: But I, Simplicio, who have made the test can assure you that a cannon ball weighing one or two hundred pounds, or even more, will not reach the ground by as much as a span ahead of a muket ball weighing only half a pound, provided both are dropped from a height of 200 cubits."

I have just read from a beautiful translation from the Latin made by Professor Henry Crew of Northwestern University who was awarded last year the Oersted Medal of the American Association of Physics Teachers for his contribution to the teaching of science.

Did Galileo drop 100 pound weights from the leaning Tower of Pisa? This question I proposed to my most distinguished teacher and col-

league, Dr. Edward A. Partridge. He answered, I don't know but I will look it up, which he did in his usual thorough way. Going over in detail all possible records of Galileo's research he found no reference by Galileo to the Tower of Pisa and so reported in *Science,* September 17, 1920. More recently there has been published by Cornell University Press, 1935, a rather complete set of the original statements accompanied by a translation of the work of Aristotle and Galileo on the research on falling bodies. Professor Lane Cooper states that Galileo makes no mention of the experiments from the leaning tower. Vincenzo Renieri, who followed Galileo in the chair of mathematics at Pisa, states in a letter to Galileo on March 13, 1641, that he had performed the experiment of testing the free fall of two different weights from the leaning tower of Pisa observing that between the ball of lead and the ball of wood there occur at least three cubits difference. No mention is made of any previous experiments.

The beautiful pictures of Galileo and the leaning tower appear to be something of a myth that has been handed down by one textbook writer after another. What ever Galileo did concerning this most important problem of free fall he at least made an impression on the minds of many who have not found time to study his original papers.

To determine the nature of the motion of the falling body Galileo rolled a hard, smooth, very round bronze ball down an inclined plane about 12 cubits long, since he believed the motion similar to that of falling freely only not so rapid. For the measurement of time, he employed a large vessel of water placed in an elevated position; to the bottom of this vessel was soldered a pipe of small diameter giving a thin jet of water, which he collected in a small glass during the descent. The water so collected was weighed on an accurate balance and the operation repeated many, many times. Such was the laboratory of physics in the latter part of the 16th Century. The result of this experiment was most fundamental. A ball rolling ¼ the length of the incline requires ½ the time of a ball rolling the entire length.

With regard to such motion Galileo established many new concepts. That the motion had been accelerated had been noted, but no one had determined the character of that acceleration. Galileo reasons, "Why should I not believe that such increases take place in a manner which is exceedingly simple and rather obvious to everybody? . . . No addition or increment more simple than that which repeats itself always in the same manner."

Roll a ball down such an incline and observe its motion. If we have an increment of two units to the velocity, during each interval of time, the ball will attain a speed of 2, 4, 6, and 8 units at the end of each interval and in consequence of this increase in speed the distance travelled during each succeeding unit of time will be in the order of 1, 3, 5, 7 units. Thus is nature's method of uniformly accelerated motion—equal

increments of speed in equal times—applied to the motions of all bodies in a good vacuum regardless of their weight. In contrast we have uniform motion; equal increments of distance in equal times.

Galileo did not fail to investigate the subject of air resistance as it affected the motion of a falling body. He reasoned: Body starts from rest and moves because of its weight and its motion is accelerated. As the velocity of fall increases so does the resistance of the air. Our falling body is now acted upon by two forces, the weight and the opposing air resistance. The resultant of these two forces gradually becomes less and less and likewise the acceleration. When the air resistance equals the weight of the body its motion becomes uniform. "The less the density of body the more quickly it becomes uniform."

This you will recognize as the terminal velocity, the greatest speed that can be attained by a falling bomb or a man without a parachute. Galileo continued his research upon the pendulum. Different weights on pendulums of the same length gave equal times of oscillation. To double the time of an oscillation we must make the pendulum four times as long.

The young Galileo believed in speaking against the accepted authority as soon as he felt convinced of the facts, tested by experiment and very soon found himself unpopular at the University of Pisa. In 1592, still only in his 28th year, after a period of only three years, he left Pisa for a professorship in mathematics at the University of Padua. The salary of a professor was small even in those days and he was obliged to do some private tutoring. All through his research he used the mathematics as a means of expressing himself and the conclusions he obtained were later tested by experiment. The use of geometry prevailed in contrast to our present algebraic analysis.

I was much interested in his treatment of the strength of beams and columns giving us the fundamental principles used in our construction of buildings and machines today. Comparing two beams of the same material, same volume, the tube has greater strength than the solid cylindrical bar.

He came upon a curious geometrical fact, that of making a cylindrical basket from a piece of matting 7 units by 25 units. Two possible baskets of very different volumes result. One is only 7 units in height and 25 units in circumference, the other, 25 units in height and 7 units in circumference.

"I really believe that, among those who are not already familiar with geometry, you would scarcely find four persons in a hundred who would not at first sight make the mistake of believing that bodies having equal surfaces would be equal in other respects."

Considerable research was carried out on the path of a projectile. "It has been observed that missiles and projectiles describe a curved path of some sort; however no one has pointed out the fact that this is a

parabola." This work laid the foundation for our present subject of exterior ballistics. Galileo considered the path as compounded from uniform motion in the direction in which the projectile is fired and an accelerated motion vertically.

Galileo must have realized the great possibilities of research which we enjoy today. To produce the parabola experimentally roll a perfectly round ball on the surface of a metallic mirror whose plane is slightly inclined to the horizontal. Wet the ball and its path will be marked upon the mirror or hang up a chain, he says, on two nails in the wall and half of the curve is a parabola. Here our philosopher made a mistake This curve, which we know as the catenary had not been carefully investigated. I also noted a reference to flying. "For I think no one believes that swimming or flying can be accomplished in a manner simpler or easier than that instinctively employed by fishes and birds." The screw propeller was apparently unknown to Galileo.

I cannot begin to point out all the lines in which his research carried him. His investigation of the velocity of light, his study of the vibration of strings in producing sounds, his construction of a thermometer place him in the front rank as a research physicist. I have called to your attention Galileo as the physicist and the mathematician. I have purposely left until the last the discussion of Galileo, the astronomer. In the light of present day astro-physics we realize how closely associated these sciences are.

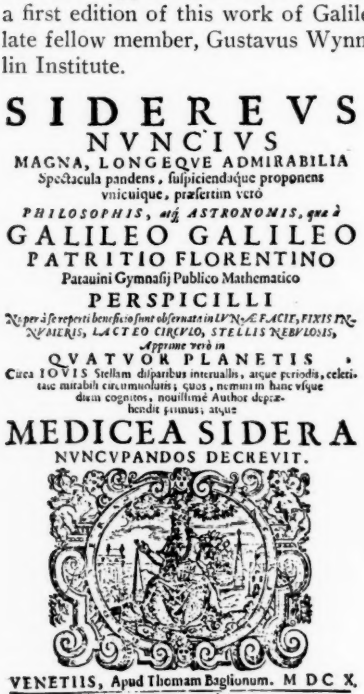
Just when this young scientist became interested in astronomy I do not know. A letter written by him to Kepler in 1597 while he was at the University of Padua states that he had adopted the Copernican theory concerning the nature of the universe. This was a big step for this young man of thirty-three. Perhaps he did not immediately begin teaching that the sun stood still and the earth was only a planet. He had trouble with the dean at Pisa and I should think he had learned a lesson. There was opposition to the newer theories. Bruno in 1600, after seven years in prison, was burned at the stake for his outspoken words against the system of Ptolemy and Aristotle. That letter to Kepler was written while Bruno was in prison.

A nova appeared in the sky in October, 1604. Such a thing was impossible in the minds of the followers of Aristotle and Ptolemy. Galileo used this opportunity to express his views from the lecture platform.

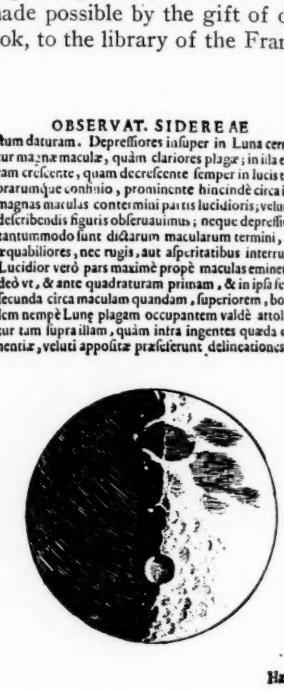
But the event in which we take the greatest interest was the construction of a peculiar tube made by a certain gentleman in Holland. Our philosopher saw the possibilities of a gadget that would make distant objects appear near. He immediately constructed such a tube placing in each end a lens and began his research. In one end he used a lens one side of which was plane and the other convex while for the eyepiece he used a lens one side plane and other concave. This first instrument magnified about nine times. Galileo constructed many other

telescopes of higher powers.

Can you recall the thrill you had when you first observed the moon through a small telescope? Perhaps it was only a ten-cent look. Something of that thrill must have been Galileo's. He did not wait long to publish his observations made during the year 1609 for his "Sidereus Nuncius" appeared in 1610. It was my privilege to examine in detail a first edition of this work of Galileo, made possible by the gift of our late fellow member, Gustavus Wynne Cook, to the library of the Franklin Institute.


SIDEREVS
NVNCIVS
MAGNA, LONGEQVE ADMIRABILIA
Spectacula pandens, suspiciendaque proponens
vnicuique, præsertim verò
PHILOSOPHIS, atq; ASTRONOMIS, quæ à
GALILEO GALILEO
PATRITIO FLORENTINO
Patauini Gymnasij Publico Mathematico
PERSPICILLI
Nuper à se reperti beneficio sunt observata in LVNÆ FACIE, FIXIS IN-
NVMERIS, LACTEO CIRCVLO, STELLIS NEBVLOSIS,
Apprime verò in
QVATVOR PLANETIS
Circa IOVIS Stellam disparibus interuallis, atque periodis, celeri-
tate mirabili circumuolutis; quos, nemini m hanc vsque
diem cognitos, nouissimè Author depræ-
hendit primus; atque
MEDICEA SIDERA
NVNCVPANDOS DECREVIT.
VENETIIS, Apud Thomam Baglionum. M DC X.
Superiorum Permissu, & Priuilegio.

TITLE PAGE OF "SIDEREUS NUNCIUS"


OBSERVAT. SIDEREAE
Cum daturam. Depressiores insuper in Luna cernuntur magnæ maculæ, quàm clariores plagæ; in illa enim tam crescente, quam decrescente semper in lucis tenebrarumque confinio, prominente hinc indè circa ipsas magnas maculas contermini partis lucidioris; veluti in describendis figuris obseruauimus; neque depressiores tantummodo sunt dictarum macularum termini, sed æquabiliores, nec rugis, aut asperitatibus interrupti. Lucidior verò pars maximè propè maculas eminet; adeò vt, & ante quadraturam primam, & in ipsa fermè secunda circa maculam quandam, superiorem, boream nempè Lunæ plagam occupantem valdè attollantur tam supra illam, quàm intra ingentes quædam eminentiæ, veluti appositæ præseferunt delineationes.

Hæc

SKETCH OF MOON BY GALILEO

Here I observed the records, the drawings of observations made more than 300 years ago by the one whose memory we commemorate. Having constructed his own telescope, Galileo turned it to observation of the heavens. "First of all," he says, "I viewed the moon." He distinguished certain "ancient spots," the dark areas visible to every one, from smaller spots sprinkled over the whole surface of the moon. "These spots have never been observed by any one before me." Why Galileo should have made this last statement I do not know. Others, perhaps, had made claims of priority. Such was the case and the battle of words began. What of it, I say, for man to quibble before the grandeur of the universe about to be exposed to the eyes of mere human beings!

The smooth surface of the moon, as considered by a large school of philosophers, was gone forever. The moon appears much like the earth; mountains and valleys, upon which the sun was shining, casting long shadows in the valleys at sunrise and crowning the higher peaks with splendor.

Galileo's sketches clearly show the terminator, the large crater-like formations so familiar to us. He says, "The grandeur, however, of such prominences and depressions in the moon seems to surpass both in magnitude and extent the ruggedness of the earth's surface."

The fixed stars and planets were found to have a difference not observed with unaided eye. He records, "The planets present their discs perfectly round—appear as so many little moons, but the fixed stars appear of same shape as when they are viewed by simply looking at them, except that a star of the 5th or 6th magnitude seems equal to Sirius." Galileo was impressed, as we have been, by the infinite multitude of telescopic stars and he had not heard of Professor Shapley's super-galaxies!

His book "Sidereus Nuncius" contains thirty numbered pages (number on one side only) with two additional leaves showing star maps. Difference in magnitude of stars is clearly shown. Galileo had intended to draw a map of the entire constellation of Orion but since he had noted more than 500 new stars within the limits of one or two degrees he decided to defer this until later. Around the three stars in Orion's belt and the six in the sword, "I have added eighty other stars." Not so bad for this early instrument which he had constructed. Another star map in the region of the Pleiades shows thirty-six stars where only six or seven are visible to the eye alone. The riddle of the Milky Way was cleared up by the irrefragable evidence of our eyes—for the galaxy is nothing else but a mass of innumerable stars arranged together in clusters.

Galileo considered the most important announcement in the "Starry Messenger" his report on the observations first made on the 7th of January, 1610, with a very excellent instrument of higher power than he had used before. Viewing the planet Jupiter in his telescope, "I noticed," he says, "three little stars—near the planet arranged exactly in a straight line parallel to the ecliptic." The position was as follows. On the east were two stars and a single one towards the west. Star furthest towards the east and the western star, appeared rather larger than the third. Note that Galileo made minute observations regarding position and magnitude but he did not realize their significance in respect to Jupiter.

His diagram:

Ori (east) * * ○ * occ. (west)

On January 8 his observations gave him much to think about; the three little stars all appeared to the west of Jupiter, nearer together and

separated by equal intervals.

○ * * * occ.

You can imagine how disappointed he must have been when January 9 was cloudy.

January 10: * * ○

He considered the third star behind the planet: "Doubt changed to surprise, I discovered that the interchange of position which I saw belonged not to Jupiter, but to the stars and I thought that they ought to be observed with more attention and precision."

Then follow 65 diagrams in his book showing relative position of the four little stars, the fourth was not observed until January 13. Our philosopher did not permit his engagements to interfere with his most careful observations.

"I, therefore, concluded and decided unhesitantingly that there are stars in the heavens moving about Jupiter, as Venus and Mercury around the sun, which at length was established as clear as daylight by numerous other observations."

Galileo called these bodies the four Medicean planets "discovered for the first time by me." He may be wrong but this is not the time to discuss such matters. Simon Marius who was observing about the same time named these four satellites Io, Europa, Ganymede, Callisto.

This telescopic view was indeed a model of the system of the world proposed by Copernicus. The planet Jupiter was the center of a system of bodies which revolved about it. All this is "clear as daylight" to us moderns of 1942 but we must remember that the Earth had been considered by many to be the only body around which others moved. The Copernican Theory was a revolution in thought prior to 1610.

Galileo's feeling in the matter can be understood from a letter to Kepler. "Oh how I wish we could have one hearty laugh together. Here at Padua, the principal professor of Philosophy refuses to look at the moon and planets through my glass. What shouts of laughter we should have at this glorious folly; to hear the professor of Philosophy at Pisa laboring before the grand duke as if to charm the new planets from the sky."

Galileo had been brought up amid such surroundings and now was to exhibit an independence of thought backed up by observation and experiment. Along with Gilbert and Des Cartes he instituted the scientific era. Examining another book by Galileo published in 1613 I found a record of observations of the positions of the moons of Jupiter for March, April, and May, 1613. This record has the same composition as that found in the *American Ephemeris and Nautical Almanac* of today with which you are no doubt familiar.

Galileo was one of the first observers of sunspots and as early as 1613 published a series of 38 full-page diagrams, about 5 inches in diameter, which I declare could not be distinguished from the beautiful work

of our own genial astronomer Rumrill* who has been a faithful observer of the sun for many years.

As the result of these most remarkable discoveries Galileo received the patronage of the Grand Duke of Tuscany, Cosmo de' Medici, and was able to give up his teaching and reside in Florence, there to enjoy the leisure and to complete his work on the universe and on the "Two New Sciences."

I wonder which side of the controversy we would have taken. Would we have been able to side with Galileo, to withstand the side glances, the casual remarks of those who upheld the older theory of the universe, especially when we found our philosopher called before the Inquisition at Rome. For years he was hindered in his work by those who refused to think, by those who failed to see in the scientific method of Galileo the only sure way of finding out the secrets which nature had so long withheld from the grasp of mankind.

I have perhaps told you very little you did not know. In this the 300th anniversary of the passing of one of the world's great researchers I have tried to present to you Galileo, mathematician, physicist, and astronomer.

*See May (1942) issue of POPULAR ASTRONOMY, p. 245.

John H. Darling
1847-1942

By CURVIN H. GINGRICH

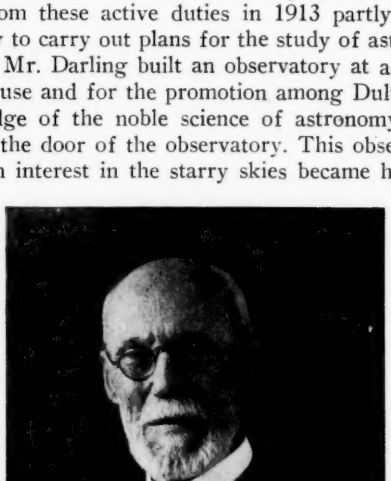
Those who have attended the meetings of astronomers more or less regularly during the last quarter of a century will remember a quiet, kindly, cultured gentleman who was frequently seen at such gatherings. It may be that he was not known by name to many, because he was of a modest and retiring nature and only reluctantly permitted himself to come into prominence. The people of Duluth, Minnesota, however, were well acquainted with him as he was an honored citizen of that community for fifty-eight years. He was so well and so favorably known by his fellow citizens that in 1930 he was made a member of the Duluth Hall of Fame.

This gentleman, John H. Darling by name, died at his home in Duluth on September 11, then in his ninety-sixth year.

Mr. Darling came to Duluth in 1884 and for the next forty years worked as an engineer in the service of the United States Government. During this period his duties included the designing and construction of breakwaters, entrance piers, channels and anchorage basins, plans for the establishment of harbor lines, inspection of bridge construction over navigable waters, location and marking of wrecks, reporting shoals and other obstructions, making preliminary examinations for im-

provements, final surveys and estimates of proposed improvements.

He retired from these active duties in 1913 partly for reasons of health and partly to carry out plans for the study of astronomy and for travel. In 1917 Mr. Darling built an observatory at a cost of $12,000 "for his private use and for the promotion among Duluth citizens of a popular knowledge of the noble science of astronomy." This is the inscription over the door of the observatory. This observatory and the inculcation of an interest in the starry skies became his chief concern



JOHN H. DARLING
1847-1942

for the rest of his life. As long as he was able to do so, he personally directed the affairs of the observatory and generously spent many evenings with groups and individuals who wished to look at the celestial objects through the nine-inch telescope. When he was no longer able to be present himself, he employed an assistant in order that the facilities for culture and education which he had begun might be continued.

In his will he made the State Teachers College of Duluth the beneficiary of the Darling Observatory, with the stipulation that it continue to be opened to the public for astronomical study.

That his interest in the science which became his hobby continued to the end of his long life is attested by the fact that in the March (1942) issue of this magazine an article by him was published.

At the age of 90 he said that, although the mortality tables granted

him less than a year and a half more, he thought he would be able to do better. At the age of 94 he expressed his conviction that "After all, extreme old age should not be the chief aim of the individual, but rather a useful life so that the world will be better for his having lived."

All who were privileged to know him and to understand him will agree that this philosophy was thoroughly exemplified in his life. All who experienced the gentleness and yet the strength of his character will be willing to say of him as was said of an old astronomer long ago, "He loved the stars too fondly to be fearful of the night."

Prediction Correlations in the Light Curves of 30 Long-Period Variable Stars

By CLINTON B. FORD

Introduction

It has long been recognized that the visual light-variations of long-period variable stars do not repeat themselves so precisely as do those of the shorter-period variables. In 1929, Eddington and Plakidis showed that, at least for o Ceti and χ Cygni, the deviations in period (maximum to maximum) do not show any special statistical distribution.[1] With the exception of certain well-known cases,[2] the same result was found to apply to 19 other long-period stars, in two subsequent papers by Plakidis.[3] However, an exhaustive study of the light curve of the irregular variable SS Cygni by Sterne and L. Campbell[4] revealed several definite associations between such quantities as length of cycle, height of neighboring maxima, steepness of ascent, etc.; and in January, 1940, Campbell announced the possible existence of two similar relations in o Ceti.[5] The distinguishing feature of the latter was that the cycles were measured not at maximum or minimum light, but at some empirically determined intermediate magnitude, not necessarily the mean. To quote Campbell,

> "A possible correlation has beeen found in the light curves of o Ceti between the varying observed magnitudes at maximum and the intervals between consecutive attainment of magnitude six on the increase to maximum, as well as the width of maximum at the same 6.0 magnitude point. When the maximum is exceptionally bright, it is preceded by a relatively short cycle between the phases at magnitude 6.0 I; when faint, the cycle is correspondingly longer. Also, for the brighter maxima the width across maximum at magnitude six is larger than for the fainter maxima . . .* Such close correlations do not appear to exist for places other than those at magnitude 6.0 I."

*In the original plotting of the first relation, cycles measured at 6.0 D were inadvertently used instead of those at 6.0 I (see *P. A.*, **48**, 307). The correct plotting is shown in Fig. 2 of the present paper.

These relations may be stated in algebraic form,

$$M_2 = b\,W + a \quad (1)$$
$$M_1 = d\,w + c \quad (2)$$

where M_1 and M_2 are the magnitudes of any two successive maxima, respectively; W is the length of a cycle at some empirically determined magnitude m_w (the same for all cycles, and equal to $6^M.0$ for o Ceti), near the median magnitude of the star, measured from the increasing branch (m_w, I point) previous to M_1 to that previous to M_2; w is the width of the light curve at m_w below M_1; and a, b, c, and d are constants. In Fig. 1 the quantities thus defined are illustrated as they might appear in a typical case.

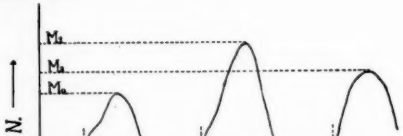


Fig. 1

In a recently received paper by P. Ahnert,[6] a study has been carried out on 29 long-period variables, in which W-intervals were measured not at an empirical magnitude m_w, but *at maximum light* (Fig. 1, from M_0 to M_1, where M_0 is height of the maximum preceding M_1), and are shown to be correlated with M_2. Ahnert separates his observed values of W (M_0 to M_1) for each star into two statistical groups, a long and a short, centered about the average value of W. The time difference between the mean W for the long group and that for the short group is then correlated with the magnitude difference between the mean magnitude of a bright and a faint group of observed maxima, the latter two groups being correspondingly centered about the (over-all) average value of the maximum magnitude. It is found that shorter W-intervals from M_0 to M_1 are generally followed by brighter maxima at M_2, and *vice versa,* longer intervals by fainter maxima at M_2. This result, based on an average of 12 observed cycles per star, agrees qualitatively with Campbell's results using $m_w = 6^M.0$ for o Ceti. However, the length of Ahnert's W-intervals must be considerably affected by the method used, and uncertainties involved, in determining the time of maximum light, while the W used by Campbell can always be fixed quite accurately, since its extremities lie on the increasing slope of each cycle. Also, Ahnert's statistical treatment does not afford a ready means of predicting maximum magnitudes from currently measured values of W. An

extension of Campbell's method to a representative list of long-period variables with considerably varying maxima, and a comparison of results with those of Ahnert, was accordingly suggested, and forms the subject of the present paper.

The Observational Material

From the extensive A.A.V.S.O. material available at Harvard College Observatory, 30 stars were selected on the basis of completeness of light curves and extent of variation of maxima and minima. In so far as possible, all degrees of the latter were considered. The intervals of time covered vary from star to star; the light curve of o Ceti was studied as far back as J.D. 2327600 (A.D. 1660), while the shortest curves are those for a group of 11 stars which were carried back only to J.D. 2423000 (1921).* No curves were plotted later than J.D. 2429000 (May, 1938). All observations were reduced to ten-day means, the writer's contribution as an A.A.V.S.O. member being 803 estimates combined in 442 ten-day places.

The selected stars, with their designations and spectral types[7] are listed in Table I, columns 1 to 3. The inclusive serial numbers of maxima observed, in column 4, follow the numbering adopted by Campbell and Sterne,[8] and the periods in column 5 are those determined by these authors.[9] Columns 6 and 7 give the mean maximum and minimum magnitudes for each star, as determined from the cycles used in the present work, and columns 8 and 9, the observed extremes in the variation of maximum magnitude. In column 10 is given the empirical magnitude m_w at which cycles were measured. Other entries in Table I will be explained later.

Table II illustrates a typical (condensed) page of data read from a light curve. Columns 1 and 2 give the times of arriving at magnitude m_w on the increasing branch of the light curve (m_w, I points), and their differences W. Columns 3 and 4 list the times of, and intervals between, maximum light, the times being estimated to the nearest 5 days from the observed shape of the curve at maximum. These estimated times were adopted because it was desirable to have each stated time of maximum actually coincide, within the limits of observational error, with the true moment of attaining the corresponding stated maximum magnitude. Maximum times derived by superimposing an entire mean light curve on individual cycles were often found to differ from such estimated times by much more than the obvious error of the latter. Columns 5 and 6 give corresponding data for the times of minimum light, column 7 contains the maximum magnitude following each W on the same line (*i.e.*, the M_2 maximum corresponding to that W in relation [1]), and column 8 the magnitude of the minimum following each maximum on the same line. In column 9, weights are assigned to each pair of values

*Either because short periods resulted in plenty of cycles already being observed, or because of scarcity of observations previous to 1921.

of W and M_2, as follows: weight 3 if both the m_w, I points determining W, and also the M_2 involved, are well determined; weights 2 and 1 if one or two, respectively, of these quantities is uncertain.

TABLE I

Design.	Star	Spec.	Max.'a Obs'd	Per.	Obs'd Max. Magn.	Obs'd Mean Min. Magn.	Obs'd Range of Max. Br.	Obs'd Range of Max. Ft.	m_w Mag.	W Aver.	Day of Max. Times	Day of Min. Times	F Rel. (1)	F Rel. (3)
				d	m	m	m	m	m	d	d	d		
001032	S Scl	M6e	69-91	365.5	6.8	12.7	0.2	7.5	9.0	20	7	10	0.15	
011272	S Cas	Se	31-45	611.8	8.7	14.9	7.8	9.9	10.5	15	19	38	0.33	
021024	R Ari	M3e	100-157	186.6	8.2	12.9	7.5	9.1	11.0	12	7	9	0.31	
021403	o Cet	M5e	10-378	331.5	3.5	9.1	2.1	5.2	6.0	10	9	14	0.32	
043274	X Cam	M3e	44-85	143.0	8.3	12.6	7.5	9.7	10.0	9	9	7	0.55	
050953	R Aur	M7e	38-59	458.2	7.8	13.3	6.8	8.5	10.0	33	17	28	0.35	
054920a	U Ori	M8e	33-51	373.2	6.3	12.0	5.3	7.6	9.0	17	13	10	0.48	
055686	R Oct	M7e	26-44	406.1	7.8	12.5	6.7	9.2	11.0	20	9	23	0.31	
060450	X Aur	M3e	49-84	163.3	8.6	12.5	8.1	9.0	10.0	9	15	7	0.00	
074241	W Pup	M3e	87-136	120.5	8.4	12.4	7.5	9.2	10.5	7	4	4	0.30	0.30
103769	R UMa	M4e	80-115	301.2	7.5	12.8	6.8	8.1	11.0	10	10	8	0.29	0.46
123160	T UMa	M4e	87-110	256.6	7.9	12.9	6.7	8.9	9.5	12	14	17	0.32	
123961	S UMa	Se	128-154	285.6	7.8	11.7	7.5	8.1	9.5	6	6	4	0.00	
132422	R Hya	M7e	201-224	var.	4.4	9.5	4.0	5.3	7.5	26	6	8	0.31	
134440	R CVn	M7e	25-53	326.2	7.7	11.8	6.9	8.3	9.0	15	11	13	0.29	
141567	U UMi	M6e	5-37	327.6	8.1	12.0	7.4	8.4	10.5	12	15	14	0.15	
151731	S CBr	M7e	46-78	360.9	7.2	12.8	6.0	8.4	9.5	11	14	20	0.17	
154615	R Ser	M7e	131-158	357.1	6.7	13.3	5.6	7.7	10.0	17	10	8	0.29	0.29
160625	RU Her	M7e	15-31	483.4	8.4	13.9	7.0	9.7	11.0	20	10	40	0.45	
161138	W CBr	M3e	49-73	238.2	8.7	13.1	7.8	9.6	11.0	25	11	11	0.22	
180531	T Her	M3e	145-181	164.5	7.9	13.0	7.2	8.8	9.5	8	10	6	0.25	0.23
190108	R Aql	M7e	51-90	var.	7.7	11.5	5.5	7.4	9.0	14	14	12	0.27	0.11
190967a	U Dra	M7e	11-48	315.2	9.5	13.9	9.1	10.4	12.5	16	13	15	0.15	
193449	R Cyg	Se	50-72	425.4	7.2	14.0	6.5	8.2	10.0	13	15	15	0.29	
194048	RT Cyg	M3e	60-91	190.4	7.4	11.9	6.5	8.2	9.5	28	16	15	0.41	
194632	χ Cyg	M5pe	203-225	406.7	4.1	13.4	4.5	6.9	10.5	8	5	5	0.29	
195849	Z Cyg	M5e	49-71	262.5	8.6	13.3	7.5	9.6	10.0	29	9	14	0.43	0.38
201008	R Del	M5e	75-95	284.0	8.3	13.2	7.8	8.8	10.5	11	6	7	0.30	
210868	T Cep	M6e	40-54	388.4	6.0	10.4	5.7	6.4	8.5	9	5	7	0.00	
235350	R Cas	M7e	44-71	429.1	6.8	12.5	5.8	8.2	9.0	12	5	6	0.05	
										10	10	19		

Discussion

At the outset, various other relations involving m_w were suggested. These may be summarized as follows:

Relation sought	Table II	Results
Length of cycle at m_w on decreasing branch *vs.* depth of following minimum.	Difference between successive m_w, D times (not tabulated finally).	Not found. (1a)
Width of minimum at m_w *vs.* depth of minimum below.	Column 1 minus m_w, D times preceding.	Very rough; not so good as relation (2). (2a)
Depth of minimum *vs.* height of maximum following.	Column 8 *vs.* column 7 following.	Holds for 6 stars (see Table III); not (3) found for others.

Relations (1a) and (2a) are simple extensions of (1) and (2) to the case of minima. Evidently both (2) and (2a) are purely geometric consequences of the shape of the average light curve, and as such we may dismiss them as being true in a rough way, but not of any special interest. However, the fact that (1a) was *not* found would indicate that its *co*-relation, (1), *is not merely geometric,* but is connected in some obscure way with the physical process of long-period variation in so far as this process is revealed by visual light.

TABLE II

SAMPLE DATA READ FROM A LIGHT CURVE

074241	W Pup		1921-38		$m_w = 10^m.5$		
10.5 I		Max.		Min.		Max. Mag.	Min. Mag.	Rel. (1) Weight
242-	W	242-	Δ	242-	Δ			
		3040		----		7.5	----	
----		(3165)	125	3225		8.9	12.6	
3246		(3285)	120	3350	125	(8.3)	(12.0)	
3364	118	3400	115	3460	110	8.0	11.7	3
3478	114	3520	120	(3580)	120	8.0	(12.3)	3
3603	125	3640	120	3705	125	(8.2)	13.6	2
3733	130	3760	120	3825	120	8.7	(13.0)	3
⋮	⋮	⋮	⋮	⋮	⋮	⋮	⋮	⋮
8656	111	8690	110	----		8.1	----	3
8785	129	8815	125	8885		9.0	12.1	3
8902	117	8940	125	8995	110	8.9	11.5	3
9013	111							

(1) and (1a) were not specifically considered by Campbell and Sterne in their study of SS Cygni, but their association of maximum brightness with the width of preceding minimum[10] corresponds to (3) above, since "width of minimum" was measured at $10^M.0$ for SS Cygni (near its median magnitude). The fact that relation (3) was found for only 6 stars must be to some extent a selective effect, since relatively few of the stars studied show conspicuous and well-observed variations in both maximum and minimum magnitude. In general, deep minima are followed by faint maxima, and *vice versa,* thus defining a third equation,

$$M_2 = e\,m + f, \tag{3}$$

where M_2 is the magnitude of any maximum, m the magnitude of the minimum preceding, and e and f are constants.

The method of determining m_w requires some explanation. At first, an unsuccessful attempt was made to derive an expression for m_w in terms of recognized characteristics of the light curve. It soon became apparent that m_w could only be determined empirically for each star. The method consisted of finding the median magnitude, and then choosing the nearest whole or half magnitude for a trial m_w. The choice here was conditioned by the general appearance of the light curve as regards symmetry, extent of variation of maxima, position of still-stands if present, and completeness of observations. No attempt was made to avoid still-stands, though their presence in some cases reduced the weight of W by making its extremities uncertain. m_w was found to fall above and below still-stand magnitudes in about equal proportion. In some light curves, the still-stand magnitude varies considerably from cycle to cycle, so that m_w necessarily fell both above and below the still-stand for an individual star.* With each trial m_w, values of W were measured from the curves and plotted against M_2. This procedure was repeated until a minimum scattering of points was obtained, or until it appeared that the suspected relation did not exist by reason of a uniform scattering of points for a number of widely different trial values of m_w. In searching for (1a), for example, a procedure involving corresponding quantities was used, and invariably led to a quite uniform scattering.

Sample plottings for relations (1) and (3) are shown in Fig. 2. The correlations are rough at best, and instead of determining the constants a, b, e, and f, it seemed best to devise an approximate representation which would include the relations and also the amount of scattering involved for each star. The method devised is explained by reference to Fig. 2c. In each M_2, W plot from the final m_w, the area covered by the plotted points was enclosed by two horizontal lines representing the brightest and faintest observed maxima, and by two or more sloping straight lines drawn through the most extreme points of weight 3. In this way, a closed area could always be defined which contained all the points of weight 3 and usually all those of weights 2 and 1. The difference between the brightest and the faintest observed maxima (Table I, column 9 minus column 8) was then taken as R, the extreme range of the observed maxima. This total range was then compared with the specific range falling within the defined area at any given value of W, say W_n, this latter specific range being called R_n. There may now be defined for each value of W a quantity F given by

$$F = 1 - R_n/R, \tag{4}$$

such that if the scattering of the plotted points is zero (perfect linear

*Cases in point are R Aur, R Hya, and U Mi.

correlation), F is equal to unity, and if the scattering is uniform (no correlation), F is zero. The value of F for any particular value of W is an indication of the accuracy of the correlation at that W.

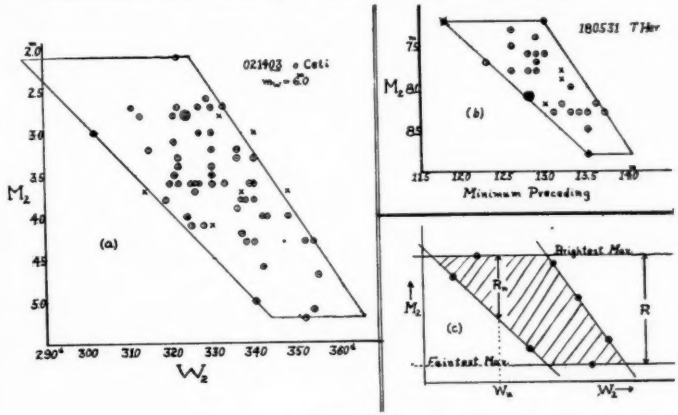


FIGURE 2

(a) RELATION (1) FOR o CETI; (b) RELATION (3) FOR T HER; (c) METHOD OF DEFINING $F = -R_n/R$. CIRCLED DOTS ARE OF WEIGHT 3, CROSSES WEIGHT 2, AND DOTS WEIGHT 1.

In Table III, the stars are listed in order of decreasing minimum values of F, and for each star the limiting magnitudes of the *predicted maximum* (*i.e.*, the limits of the range R_n) are tabulated for a convenient set of values of W. Entering Table III with a currently observed W, we may now take out the limits of magnitude between which the next maximum will occur, and in a final column, the value of F for this prediction. A second section of Table III gives corresponding data for stars obeying relation (3).

Returning to Table I we find in the final column the minimum value of F for each star, designated F_m. Three of the stars, X Aur, S UMa, and T Cep, have $F_m = 0$, and the existence of relation (1) seems very doubtful for R Cas ($F_m = 0.05$). The highest F_m is 0.55 for X Cam, and the mean for all the stars is 0.27 by relation (1). There seems to be no connection with spectral type, period, or other known characteristics, the three Se stars in the list, for example, showing extreme values of F_m. There is, however, a rough relation between average range of variation (Table I, column 7 minus column 6) and F_m, high values of F_m going with high ranges and *vice vesra*.

It is generally recognized that the asymmetry of long-period light curves progresses with the period. Thus, if (1) were merely geometric, we might expect to find its accuracy (F_m) falling off with increasing period. The fact that this is not the case suggests again that (1) is a characteristic effect of long-period variation in so far as the latter is

manifested by visual light. Such a notion is given some further weight by a consideration of the average deviations of W and of the times of maximum light, as tabulated in Table I, columns 11 to 13. If relation (1) were exact and solely geometric, we should expect the mean deviation of W (column 11) to vary directly with the three factors which determine it, namely, the range of maximum magnitude (column 9

TABLE III

FOR THE PREDICTION OF MAXIMA BY RELATION (1)

W	Limits of Pred. Max. Ft.	Limits of Pred. Max. Br.	F
X Cam R=2.^{m}2 m_W=10.^{m}0			
120	8.4	7.5	0.59
125	8.4	7.5	.59
130	8.4	7.5	.59
135	8.5	7.6	.59
140	8.5	7.6	.59
145	8.5	7.7	.67
150	8.7	7.8	.59
155	8.9	7.9	.55
160	9.2	8.3	.59
165	9.5	8.6	.59
170	9.7	8.9	.59
175	9.7	8.9	.77
180	9.7	9.6	0.95
U Ori R=2.^{m}3 m_W=9.^{m}0			
340	6.0	5.3	0.70
345	6.2	5.3	.61
350	6.4	5.3	.52
355	6.5	5.4	.52
360	6.7	5.5	.48
365	6.8	5.7	.52
370	6.9	5.8	.52
375	7.0	5.9	.52
380	7.0	6.1	.61
385	7.1	6.2	.61
390	7.2	6.3	.61
395	7.3	6.5	.65
400	7.4	6.6	.65
405	7.5	6.7	.65
410	7.6	6.9	.70
415	7.6	7.0	.74
420	7.6	7.1	0.78
RU Her R=2.^{m}7 m_W=11.^{m}0			
440	7.2	7.0	0.93
450	7.5	7.0	.81
460	7.9	7.0	.67
470	8.2	7.0	.56
480	8.6	7.2	.48
490	8.9	7.5	.48
500	9.3	7.8	.45
510	9.6	8.2	.48
520	9.7	8.5	.56
530	9.7	8.8	.67
540	9.7	9.1	.78
550	9.7	9.5	0.93

W	Limits of Pred. Max. Ft.	Limits of Pred. Max. Br.	F
Z Cyg R=2.^{m}1 m_W=10.^{m}0			
235	7.5	7.5	1.00
240	7.8	7.5	.86
245	8.2	7.6	.71
250	8.6	7.7	.57
255	8.9	7.9	.52
260	9.1	8.0	.48
265	9.3	8.2	.48
270	9.5	8.3	.43
275	9.6	8.4	.43
280	9.6	8.5	.48
285	9.6	8.7	.57
290	9.6	8.8	0.62
RT Cyg R=1.^{m}7 m_W=9.^{m}5			
170	7.1	6.5	0.65
175	7.3	6.5	.53
180	7.6	6.6	.41
185	7.8	6.8	.41
180	8.0	7.0	.41
195	8.2	7.2	.41
200	8.2	7.4	.53
205	8.2	7.5	.59
210	8.2	7.7	.71
215	8.2	7.9	0.82
R Aur R=1.^{m}7 m_W=10.^{m}0			
390	7.5	6.8	0.59
400	7.6	6.8	.53
410	7.7	6.8	.47
420	7.8	6.8	.41
430	7.9	6.8	.35
440	7.9	6.8	.35
450	8.0	6.9	.35
460	8.1	7.0	.35
470	8.2	7.1	.35
480	8.2	7.2	.41
490	8.3	7.3	.41
500	8.4	7.4	.41
510	8.5	7.5	.41
520	8.5	7.6	.47
530	8.5	7.7	.53
540	8.5	7.8	.59
550	8.5	7.9	0.65

W	Limits of Pred. Max. Ft.	Limits of Pred. Max. Br.	F
S Cas R=2.^{m}1 m_W=10.^{m}5			
595	9.0	7.8	0.43
600	9.1	7.8	.38
605	9.1	7.8	.38
610	9.2	7.8	.33
615	9.2	7.8	.33
620	9.3	8.0	.38
625	9.4	8.1	.38
630	9.4	8.2	.43
635	9.5	8.4	.48
640	9.5	8.5	.52
645	9.6	8.7	.57
650	9.6	8.8	.62
655	9.7	9.0	.67
660	9.7	9.1	.71
665	9.8	9.2	.71
670	9.9	9.4	.76
675	9.9	9.5	.81
680	9.9	9.7	.90
685	9.9	9.8	.95
690	9.9	10.0	0.95
o Cet R=3.^{m}1 m_W=6.^{m}0			
300	2.9	2.1	0.74
305	3.1	2.1	.68
310	3.4	2.1	.58
315	3.7	2.1	.48
320	4.0	2.1	.39
325	4.2	2.1	.32
330	4.5	2.5	.36
335	4.7	2.8	.39
340	5.0	3.2	.42
345	5.2	3.6	.48
350	5.2	4.0	.61
355	5.2	4.4	.74
360	5.2	4.7	0.84
T UMa R=2.^{m}2 m_W=9.^{m}5			
230	8.0	6.7	0.41
235	8.1	6.7	.36
240	8.2	6.7	.32
245	8.3	6.8	.32
250	8.4	6.9	.32
255	8.5	7.1	.36
260	8.6	7.2	.36
265	8.7	7.3	.36
270	8.8	7.6	0.45

minus column 8), the average deviation of maximum times (column 12), and the average deviation of minimum times (column 13). A plot of the product of these three quantities against the mean deviation of W in all cases reveals a random scattering of points. If this scattering were entirely due to the roughness of relation (1), stars with

TABLE III
(Second Part)

W	Limits of Pred. Max. Ft.	Limits of Pred. Max. Br.	F
T UMa	R=2.2	m_W = 9.5	
275	8.9	8.0	0.59
280	8.9	8.3	0.73
κ Hya	R=1.3	m_W = 7.5	
330	4.0	4.0	1.00
340	4.1	4.0	.94
350	4.2	4.0	.85
360	4.3	4.0	.77
370	4.4	4.0	.71
380	4.5	4.0	.64
390	4.6	4.0	.57
400	4.7	4.0	.46
410	4.8	4.0	.38
420	4.9	4.0	.31
430	5.0	4.1	.31
440	5.1	4.3	.38
450	5.2	4.4	.38
460	5.3	4.6	.46
470	5.3	4.7	.57
480	5.3	4.8	.64
490	5.3	5.0	.77
500	5.3	5.1	0.85
R Ari	R=1.6	m_W=11.0	
150	7.8	7.5	0.81
155	8.0	7.5	.69
160	8.1	7.5	.63
165	8.3	7.5	.50
170	8.4	7.6	.50
175	8.5	7.7	.50
180	8.7	7.7	.38
185	8.8	7.8	.38
190	9.0	7.9	.31
195	9.1	8.0	.31
200	9.1	8.1	.38
205	9.1	8.2	.44
210	9.1	8.3	.50
215	9.1	8.3	.50

W	Limits of Pred. Max. Ft.	Limits of Pred. Max. Br.	F
R Oct	R=2.6	m_W=11.0	
350	7.4	6.7	0.73
355	7.5	6.7	.69
360	7.6	6.9	.65
365	7.7	6.7	.62
370	7.9	6.7	.54
375	8.0	6.7	.50
380	8.1	6.7	.46
385	8.2	6.7	.42
390	8.4	6.7	.35
395	8.5	6.7	.31
400	8.6	6.8	.31
405	8.7	6.9	.31
410	8.8	7.0	.31
415	9.0	7.2	.31
420	9.1	7.3	.31
425	9.2	7.4	.31
430	9.2	7.5	.35
435	9.2	7.6	.39
440	9.2	7.7	.42
445	9.2	7.8	.46
W Pup	R=1.7	m_W=10.5	
105	7.5	7.5	1.00
110	8.2	7.6	.65
115	8.9	7.7	.30
120	9.0	7.8	.30
125	9.0	7.9	.35
130	9.1	8.2	.47
135	9.2	8.9	.82

W	Limits of Pred. Max. Ft.	Limits of Pred. Max. Br.	F
R Del	R=1.0	m_W=10.5	
270	8.0	7.8	0.80
275	8.1	7.8	.70
280	8.3	7.8	.50
285	8.5	7.9	.40
290	8.7	8.1	.30
295	8.8	8.3	.50
300	8.8	8.5	.70
305	8.8	8.6	.80
R Cyg	R=1.7	m_W=10.0	
360	6.7	6.5	0.88
370	6.9	6.5	.76
380	7.0	6.5	.71
390	7.2	6.5	.59
400	7.3	6.6	.59
410	7.5	6.6	.47
420	7.6	6.7	.47
430	7.8	6.8	.41
440	7.9	6.9	.41
450	8.1	6.9	.29
460	8.2	7.0	.29
470	8.2	7.5	.59
480	8.2	7.9	0.82
χ Cyg	R=2.4	m_W=10.5	
350	5.0	4.5	0.79
360	5.1	4.5	.75
370	5.3	4.5	.67
380	5.5	4.5	.58
390	5.7	4.5	.50
400	5.9	4.6	.46
410	6.0	4.7	.46
420	6.2	4.8	.46
430	6.4	4.8	.33
440	6.6	4.9	.29
450	6.7	5.7	.58
460	6.9	6.8	.96
R Ser	R=2.1	m_W=10.0	
310	6.4	5.6	0.62
320	6.6	5.6	.52
330	6.9	5.6	.43
340	7.0	5.6	.33

relatively high F_m-values should not give such widely scattered plots as others, but should show a trend toward a single line or curve. No such trend can be detected, so it appears that our original premise that relation (1) is solely geometric is probably false.

Ahnert considers his results, using W's measured across maxima, to be purely statistical and not applicable to individual stars. Results

TABLE III
(Third Part)

W	Limits of Pred. Max. Ft.	Limits of Pred. Max. Br.	F
R Ser	$R=2^m.1$	$m_W=10^m.0$	
350	7.2	5.7	0.29
360	7.5	6.0	.29
370	7.7	6.2	.29
380	7.7	6.5	.43
390	7.7	6.7	.52
400	7.7	7.0	.67
R CVn	$R=1^m.4$	$m_W=9^m.0$	
290	7.8	6.9	0.36
295	7.8	6.9	.36
300	7.9	6.9	.29
305	7.9	6.9	.29
310	8.0	7.0	.29
315	8.0	7.1	.36
320	8.0	7.1	.36
325	8.1	7.1	.29
330	8.1	7.2	.36
335	8.2	7.2	.29
340	8.2	7.3	.36
345	8.2	7.3	.36
350	8.3	7.3	.29
355	8.3	7.4	.36
360	8.3	7.4	.36
R UMa	$R=1^m.4$	$m_W=11^m.0$	
275	7.1	6.8	0.79
280	7.3	6.8	.64
285	7.5	6.9	.57
290	7.7	7.0	.50
295	7.9	7.1	.43
300	8.1	7.1	.29
305	8.1	7.2	.36
310	8.1	7.3	.43
315	8.1	7.4	.50
320	8.1	7.5	.57
325	8.1	7.5	.57

W	Limits of Pred. Max. Ft.	Limits of Pred. Max. Br.	F
R Aql	$R=1^m.5$	$m_W=9^m.0$	
270	5.7	5.5	0.87
280	5.9	5.6	.80
290	6.2	5.6	.60
300	6.5	5.7	.47
310	6.7	5.8	.40
320	7.0	5.9	.27
330	7.0	5.9	.27
340	7.0	6.0	.33
350	7.0	6.1	.40
T Her	$R=1^m.6$	$m_W=9^m.5$	
150	7.7	7.2	0.69
155	7.9	7.2	.56
160	8.2	7.2	.38
165	8.4	7.2	.25
170	8.7	7.5	.25
175	8.8	7.8	.38
180	8.8	8.1	.56
185	8.8	8.4	.75
W CBr	$R=1^m.8$	$m_W=11^m.0$	
200	8.0	7.8	0.89
205	8.2	7.8	.78
210	8.4	7.8	.67
215	8.6	7.8	.56
220	8.8	7.8	.44
225	9.0	7.8	.33
230	9.2	7.9	.28
235	9.5	8.1	.22
240	9.6	8.2	.22
245	9.6	8.3	.28
250	9.6	8.5	.39
255	9.6	8.6	.44
260	9.6	8.7	.50
265	9.6	8.9	.61
270	9.6	9.0	.67
275	9.6	9.2	0.78

W	Limits of Pred. Max. Ft.	Limits of Pred. Max. Br.	F
S CBr	$R=2^m.4$	$m_W=9^m.5$	
325	6.2	6.0	0.92
330	6.5	6.0	.79
335	6.8	6.0	.67
340	7.0	6.0	.58
345	7.3	6.0	.46
350	7.5	6.0	.38
355	7.8	6.0	.25
360	8.0	6.1	.21
365	8.3	6.3	.17
370	8.4	6.6	.25
375	8.4	6.9	.38
380	8.4	7.1	.46
385	8.4	7.4	.58
390	8.4	7.6	0.67
U UMi	$R=1^m.3$	$m_W=10^m.5$	
300	7.6	7.4	0.85
305	7.9	7.4	.64
310	8.2	7.5	.46
315	8.5	7.5	.23
320	8.5	7.5	.23
325	8.6	7.6	.23
330	8.7	7.6	.15
335	8.7	7.7	.23
340	8.7	7.7	.23
345	8.7	7.8	.31
350	8.7	7.9	.38
355	8.7	8.0	.46
360	8.7	8.1	.57
365	8.7	8.2	.64
370	8.7	8.3	.71
375	8.7	8.4	.77
380	8.7	8.5	0.85
U Dra	$R=1^m.3$	$m_W=12^m.5$	
280	9.5	9.1	0.71
290	9.6	9.1	.64
300	9.7	9.1	.57
310	9.9	9.1	.38
320	10.0	9.1	.31
330	10.2	9.1	.15
340	10.3	9.4	.31
350	10.4	9.6	.38
360	10.4	9.9	.64
370	10.4	10.2	0.85

derived from the more precise W's used in the present work should perhaps be similarly regarded; however, the practical value of Table III for prediction of maxima of individual stars should not be overlooked. Plots similar to those in Fig. 2, but using (estimated) M_0 to M_1 values of W, as per Ahnert, were made for a group of ten of the stars used here. The scattering in such plots was always larger than in the corresponding plots using W's measured at m_w. On the other hand, if one uses maximum times derived by superimposing a mean light curve (instead of the estimated times as in Table II), the situation is not improved. The mean light curve tends to make all cycles equal in length, and the result is a dispersion in W which is too small to show any correlation with M_2.

Summary and Conclusion

The empirical correlations discovered by L. Campbell in the visual light curve of *o* Ceti have been extended and studied in 29 other long-period variables. One of the relations (height of maximum *vs.* width of the same maximum) has been noted as merely geometric, while the other, which affords some improvement in the prediction of maximum

TABLE III
(Fourth Part)

W	Limits of Pred. Max. Ft.	Limits of Pred. Max. Br.	F
S Scl	R=1.3	m_w= 9.0	
310	6.5	6.2	0.77
320	6.6	6.2	.71
330	6.8	6.2	.57
340	7.0	6.2	.38
350	7.2	6.2	.23
360	7.4	6.3	.15
370	7.5	6.5	.23
380	7.5	6.6	.23
390	7.5	6.8	.46
400	7.5	7.0	.64
410	7.5	7.2	0.77
R Cas	R=2.4	m_w= 9.0	
395	6.5	5.8	0.71
400	6.8	5.8	.58
405	7.0	5.8	.50
410	7.2	5.8	.42
415	7.5	5.8	.29
420	7.7	5.8	.21
425	7.9	5.8	.13
430	8.1	5.8	.05
435	8.2	5.9	.05
440	8.2	6.4	.25
445	8.2	6.9	.46
450	8.2	7.5	.71
455	8.2	8.0	.92

W	Limits of Pred. Max. Ft.	Limits of Pred. Max. Br.	F
X Aur	R=0.8	m_w=10.0	
140	8.4	8.1	0.63
145	8.6	8.1	.37
150	8.7	8.1	.25
155	8.9	8.1	.00
160	9.0	8.2	.00
165	9.0	8.3	.13
170	9.0	8.4	.25
175	9.0	8.5	.37
180	9.0	8.6	.50
185	9.0	8.7	.63
190	9.0	8.7	0.63
S UMa	R=0.6	m_w= 9.5	
210	8.1	7.5	0.00
220	8.1	7.5	.00
230	8.1	7.6	.17
235	8.1	7.7	.33
240	8.1	7.7	.33
245	8.1	7.8	.50
250	8.1	7.9	.67

W	Limits of Pred. Max. Ft.	Limits of Pred. Max. Br.	F
T Cep	R=0.7	m_w= 8.5	
375	6.0	5.7	0.57
380	6.1	5.7	.43
385	6.2	5.7	.29
390	6.3	5.7	.14
400	6.3	5.7	.14
410	6.4	5.7	.00
420	6.4	5.7	.00

magnitudes, has been studied in detail and found to hold for all but four of the stars investigated. The latter relation may be stated as follows: a relatively long cycle measured at an empirically determined magnitude m_w (same for all cycles) on the increasing branch of the light curve, is generally followed by a relatively faint maximum, and *vice versa*, a relatively short cycle thus measured is usually followed by a bright maximum. For six of the stars it was also found that faint minima are usually followed by faint maxima, and *vice versa*. The results are compiled in a form (Table III) which enables one to predict readily the range of magnitude within which a future maximum will occur, and to estimate the accuracy of such a prediction.

TABLE III

FOR THE PREDICTION OF MAXIMA BY RELATION (3)

(Fifth Part)

Preceding Min.	Limits of Pred. Max. Ft.	Br.	F
R UMa	R = 1.3^m		
12.3	7.3	6.8	0.64
12.5	7.6	6.9	.46
12.7	7.8	7.1	.46
12.9	8.0	7.3	.46
13.1	8.1	7.5	.57
13.3	8.1	7.7	.71
13.5	8.1	7.9	.85
Z Cyg	R = 2.1^m		
12.6	8.3	7.5	0.62
12.8	8.5	7.5	.52
13.0	8.7	7.5	.43
13.2	8.8	7.5	.38
13.4	9.0	7.7	.38
13.6	9.1	7.9	.43
13.8	9.3	8.2	.48
14.0	9.4	8.4	.52
14.2	9.5	8.6	.57
14.4	9.6	8.9	.67
14.6	9.6	9.1	0.76
W Pup	R = 1.7^m		
11.6	7.8	7.8	1.00
11.8	8.2	7.8	0.76
12.0	8.7	7.8	.47
12.2	8.9	7.9	.41
12.4	9.0	7.9	.35
12.6	9.1	7.9	.30
12.8	9.2	8.0	.30
13.0	9.2	8.0	.30
13.2	9.2	8.3	.46
13.4	9.2	8.5	.59
13.6	9.2	8.7	0.71

Preceding Min.	Limits of Pred. Max. Ft.	Br.	F
R Ser	R = 2.1^m		
13.0	6.6	5.6	0.52
13.2	6.8	5.6	.43
13.4	6.9	5.6	.38
13.6	7.1	5.6	.29
13.8	7.3	6.2	.48
14.0	7.5	6.8	.67
14.1	7.5	7.1	.80
14.2	7.6	7.4	.89
14.3	7.7	7.7	1.00
T Her	R = 1.3^m		
11.8	7.2	7.2	1.00
12.0	7.4	7.2	.85
12.2	7.6	7.2	.71
12.4	7.7	7.2	.64
12.6	7.9	7.2	.46
12.8	8.1	7.2	.31
13.0	8.2	7.2	.23
13.2	8.3	7.5	.38
13.3	8.4	7.7	.46
13.4	8.4	7.8	.57
13.5	8.5	8.0	.64
13.6	8.5	8.2	.77
13.7	8.5	8.3	0.85
R Aql	R = 1.3^m		
10.9	5.7	5.7	1.00
11.1	6.1	5.7	.71
11.3	6.4	5.8	.57
11.5	6.8	5.8	.23
11.6	7.0	5.8	.11
11.7	7.0	6.1	.31
11.8	7.0	6.5	.64
11.9	7.0	6.8	0.85

Comparison with a similar paper by Ahnert, based on 29 stars, indicates that cycles measured at m_w give better correlations than those used by Ahnert, which were measured between successive maxima. Some evidence is accumulated which favors the belief that the m_w correlation represents more than a mere geometric property of the average long-period light curve, though the underlying cause of the correlation remains obscure.

The writer acknowledges generous help and suggestions during the preparation of this paper from Mr. Campbell of Harvard College Observatory, who made the topic available, and from Dr. D. B. McLaughlin and the late Dr. H. D. Curtis of the University of Michigan Observatory.

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[1] Eddington and Plakidis, *M. N.*, **90**, 65 (1929).

[2] Notably R Hydrae and R Aquilae, both of which show continuously decreasing periods.

[3] Plakidis, *M. N.*, **92**, 460 (1932); **93**, 373 (1933).

[4] Sterne and L. Campbell, *H. A.*, **90**, 189 (1934).

[5] Campbell, *P. A.*, **48**, 37 (1940).

[6] Ahnert, *A. N.*, **271**, 417 (1940).

[7] From C. P. and S. Gaposchkin's "Variable Stars," pp. 136-152.

[8] *Harvard Tercentenary Papers*, No. 24 (1936).

[9] *Ibid.*

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SMITH COLLEGE OBSERVATORY, MAY, 1942.

Detailed Description of the Zodiacal Light

By J. B. PENNISTON

The Zodiacal Light is a faint visual object in temperate zones, lacking in detail; and, requiring as it does no special equipment for its observation, it is scarcely a fit subject of investigation for the usual observatory staff. In addition there has been published only one series of observations worthy of any special mention, those of the Rev. George Jones, "Observations of the Zodiacal Light from April 2, 1853 to April 22, 1855," published as the third volume of M. C. Perry's report, *United States Japan Expedition*, 1856.—Jones was a chaplain of that expedition.—According to Dr. W. E. Glanville, copies of this work are quite rare. For the various reasons, just stated, the Zodiacal Light represents a celestial phenomenon decidedly neglected, and I have yet to see a description of it in any textbook or reference work that I consider entirely satisfactory. In the description which follows I use the terminology as well as the authority of the Rev. Mr. Jones except in instances where I specially refer to others or to my own observations.

1. *Shape and Location*—The Zodiacal Light is a lens- or disc-shaped beam of light (sometimes described less accurately as cone-shaped or

triangular), extending obliquely upwards from the horizon. It follows the ecliptic almost exactly in the tropics but later observations show it to lie a little north of the ecliptic in the North Temperate Zone and a little south of it in the South Temperate Zone. It is seen in the western sky after sunset and in the eastern sky before sunrise.

2. *Intensity, Appearance, and Nature of the Light*—In the tropics the Light is very conspicuous under favorable weather conditions; parts of it so much so as to be sometimes mistaken for the rising sun; yet stars can be seen shining through even the brightest part of the Light. As to the appearance of the Light, Jones says (observation No. 131) "... The first appearance of the Zodiacal Light seems to be a white light—*i.e.*, when the twilight is not fully gone; afterwards it changes to a warm yellowish light. The reverse of this happens in the morning . . ." This suggests, of course, a Doppler effect due to the earth's motion in relation to the Light. The investigations of Dr. E. A. Fath at Lick Observatory leave little doubt that the light itself is mostly reflected sunlight, and about 15% of this light is polarized as was pointed out by Dr. E. O. Hulburt[1] in his recent investigations.

3. *The Diffuse Light*—In the introduction to his work (p. xi) Jones says, "There was no mention, in any books on the Zodiacal Light, of any difference in the Light itself; . . ." It is here that textbooks are still deficient. The Light (in the tropics) is not uniform in intensity for the outer and larger proportion of the Light is dimmer or more diffuse. The lines of separation of all parts of the Zodiacal Light from each other and from the sky itself are not sharply defined but there are narrow zones in which the change of intensity is relatively rapid.

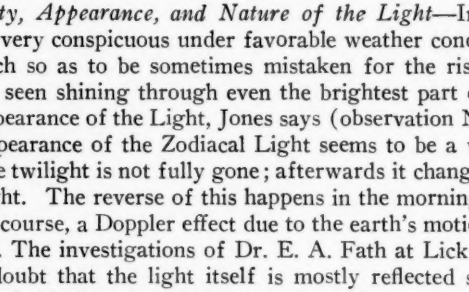

ILLUSTRATION OF THE ZODIACAL LIGHT

This illustration attempts only to show the approximate proportions and shapes of the different parts of the Zodiacal Light and its relation to the horizon. Details mentioned in section 9 are omitted.

4. *The Stronger Light*—Within the limits of the Diffuse Light and lying along the same axis but nearer the horizon is an area of stronger or brighter light, possessing the same general outline as the Diffuse Light, *i.e.*, an approximate cone-shape, sometimes referred to as the "inner cone."

5. *The Effulgent Light*—This part of the Light lies very close to the horizon, and is generally somewhat irregular in shape although Jones notes that at times it assumes an approximate cone-shape, the same as the Diffuse Light and the Stronger Light. From the intensity of its glow (*i.e.,* in the tropics) it is well named, and even in temperate zones this part of the Light shows a trifle brighter than the rest. The outline of the Effulgent Light, separating it from the Stronger Light, is very poorly defined, much more so than that between the Stronger Light and the Diffuse Light.

6. *The Paled Sky*—The parts of the Light thus far described are easily visible in the tropics, but the Paled Sky requires rather good eyesight. It is a paling or dimming of the sky in regions beyond the vertex of the Diffuse Light and for some distance along its sides. Jones noted the condition in a number of observations and I have personally noted it in the tropics. To me it suggests the possible existence of yet another "cone" of light outside the Diffuse Light, the outline of which is not quite visible.

7-8. *Zodiacal Light Bands and Gegenschein*—These phenomena seem to be accurately described in most texts. The narrow Bands are joined to the vertices of both the East and West Zodiacal Lights, and from thence they pass around the earth and meet to form a vortex (according to Dr. F. R. Moulton) at a point directly opposite from the sun, which vortex is known as the Gegenschein or Counterglow. The Gegenschein varies somewhat in shape and in size, sometimes nearly round and sometimes oval; it is a very dim visual object, and usually not to be seen near the Milky Way because of light interference. Where the Bands join the Zodiacal Light proper, the intensity of the light, according to my observations, is about the same as that of the Diffuse Light but they gradually grow dimmer as the distance increases from the vertices.

9. *Other Detail*—From personal observations in Costa Rica in 1914 I found a plainly visible detail of the Stronger Light unmentioned, so far as I know, by any other observer. The Stronger Light appeared as if it might consist of two "cones" superimposed on each other without the coincidence being quite complete. In addition I noted very faint indications of knots or twists in parts of the Stronger Light although most of it seemed to be uniform in appearance.

10. *Moon Zodiacal Light*—Observations nos. 329 to 341 inclusive in Jones' work deal with Zodiacal Light formed by the moon, a fact well attested by others. The times visible in Jones' observations vary on days from when the moon rose at 6:55 P.M. up to when it rose at 12:20 A.M. The Moon Zodiacal Light breaks bounds and disappears, of course, very shortly after the moon actually rises. Generally the Moon Zodiacal Light covers much less of the sky than the Sun Zodiacal Light. Jones (p. xviii) specifically rejected the proposition that the Zodiacal Light might

be an atmospheric phenomenon largely on the basis of his observations of the Moon Zodiacal Light.

11. *Joint Sun and Moon Zodiacal Light*—Jones gives at least two instances (observations nos. 125 and 267) where the light of the moon apparently re-enforced that of the sun to produce a Light much stronger than usual, surpassing even the moonlight itself (observation no. 267). This Joint Light, according to Jones, indicated a closer relationship to the sun than to the moon, and this fact, too, would be somewhat opposed to Barnard's atmospheric theory.[2]

12. *Pulsations*—There are three classes of pulsations. (1) A study of Jones' charts shows that there may be certain long-period oscillations, imperceptible to the naked eye but easily detected by plotting the outlines of the Light and then by re-plotting them at a later time. These changes in boundaries involve the whole Zodiacal Light. (2) There are also rapid pulsations with full periods varying from four to ten minutes, involving changes both in intensity and in boundaries of the Stronger Light and of the Effulgent Light, although Jones' observations seem to have been concerned mostly with changes in the Stronger Light. His observation no. 131 is a good example of the effects as he noted them. (3) In observation no. 119 Jones says "At 7 :40, the *brightness* pulsates, but I cannot perceive any pulsations in the *boundaries*." This observation and also those of nos. 277, 278, and 279 seem to indicate the presence of very rapid pulsations, so rapid that their duration can not be determined by the eye-and-watch method. It would be of interest to obtain a spectroscopic examination of these very rapid pulsations.

13. *Interrelation with Other Phenomena*—Dr. Hulburt[3] brought up recently the proposition that the Zodiacal Light and auroral manifestations might be related phenomena, the idea seeming to depend largely upon the fact that both phenomena are subject to disturbances by magnetic storms. The aurora is an atmospheric phenomenon while Dr. Hulburt would place the Zodiacal Light substance about 40,000 miles away. But, if magnetic storms originate in solar disturbances—and for this there is considerable evidence, it is not difficult to understand that the disturbance may involve all parts of the solar system.

14. *Celestial Analogies* — (1) *Saturn's Rings* — This phenomenon seems to have suggested the origin of the idea that the Zodiacal Light might be a ring of nebulous matter around the earth. Jones himself advanced this theory, but the analogy is not a good one in this connection as Saturn's Rings seem to be quite uniform in dimensions as they pass from one side of the planet to the other. That the supposed earth-ring widens from the Zodiacal Light Bands to a large bulge on the side towards the sun has been urged as being accounted for on the basis of a tidal bulge produced by the sun, but this reasoning is unsound because, if there is any tidal effect, the greater effect would be produced by the

moon. The analogy of Saturn's Rings seems to me to be applied with more reason when we consider the Zodiacal Light as a ring around the sun; at least the detail of Saturn's Rings suggests in part that of the Zodiacal Light. (2) *Nebulae*—Among nebulae there are examples both of ring-shaped or annular formations and also of disc-like formations. The thought suggests itself that under differing conditions the Zodiacal Light might be either a ring around the sun or a disc- or lens-shaped envelope around the sun. Laplace, Schubert, Arago, Biot, and Humboldt[4] accepted the former explanation while Sir John Herschel[5] and others prefer the latter. (3) *Sun's Corona*—The question also arises as to what relation the Zodiacal Light may bear, if any, to the sun's corona. (4) *Novae*—The most important analogy is quite possibly that of novae which involve bodies in the same class as the sun itself. The spectra of fading novae quite commonly exhibit an aspect more or less disc-like. The question arises, may not a nova be a sort of magnified, intensified and greatly disturbed Zodiacal Light surrounding a star. When a nova has changed back visually into a star it retains for some time a very faint nebula around it that suggests strongly the explanation of the Zodiacal Light offered by Herschel and other astronomers.

Conclusion—I have been impelled to write this description of the Zodiacal Light by recent theories advanced, urging the Light either as earth-ring or as an atmospheric phenomenon. I can not see that either theory accounts for the shape and detail of the Light. On the other hand the theories of it as a ring around the sun or as a disc-like envelope of that body strike me as much more reasonable. In an article[6] I suggested that the solar system might be organized somewhat on the order of the Schrödinger atom, and that suggestion fits in, of course, with the explanation of the Zodiacal Light as a lens-shaped envelope around the sun as an actual visible illustration of the "hump" which I have assumed to exist. At any rate the first step towards understanding any phenomenon is its accurate and complete description *from the best view obtainable.* Upon my first view of the Zodiacal Light in the tropics I speculated that it might be an important clue to the organization of space within the solar system and subsequent researches have only served to strengthen that opinion. To increase our knowledge of this neglected phenomenon two things, according to my viewpoint, should be done: first, a regular Zodiacal Light Observation Station should be established in the tropics; secondly, the Rev. George Jones' observations should be reprinted in order to make them available for a more general distribution.

Note on Zodiacal Light Illustrations—There are not many illustrations of the Zodiacal Light. There is one after a photograph on the wall of a lecture room at the University of Missouri and in W. F. Denning, "The Zodiacal Light and the Inferior Planets," *Hutchinson's Splendour of the Heavens,* Vol. I, there are illustrations after photographs on pages 179, 181, and two on page 185. (By error the illustration at top of page 185 is reversed, right for left.) Photographs of the Zodiacal Light by reason of long exposure unduly obscure the detail and

hence are very poor illustrations. The best illustration that I have seen is after a drawing by M. Eiffler on page 184 of the work last mentioned, but even this shows the outline too sharply defined.—J.B.P.

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SEATTLE, WASHINGTON.

Mars and Jupiter at Solar Opposition

By R. B. WEITZEL

The mean interval between oppositions of Mars and the sun, or synodic revolution, is 779.936 days, that of Jupiter is 398.884 days; thus the respective oppositions are of frequent occurrence. It is rare however for both planets to be in opposition to the sun at approximately the same time.

When Mars and Jupiter are at solar opposition within less than four and a half days of each other, there are usually, but not invariably, three geocentric conjunctions of the planets during direct, retrograde, and redirect motion, within a period of five months.

If the solar opposition of Mars *precedes* that of Jupiter, then two of the planetary conjunctions in geocentric longitude will be before and one after the oppositions. For example, a solar opposition of Mars occurred on February 10, 1790, four days before that of Jupiter on February 14, 1790. There were three geocentric conjunctions of the planets on the following dates, Greenwich civil time, at given longitudes:

		h	° ′
1789	XII	21, 21	λ 151 17
1790	I	21, 11	149 22
1790	V	6, 14	141 53

If the solar opposition of Mars *follows* that of Jupiter, then one of the planetary conjunctions in geocentric longitude will be before and two after the oppositions. For example, a solar opposition of Mars occurred on February 6, 1837, four days after that of Jupiter on February 2, 1837. There were three geocentric conjunctions of the planets on the following dates G.C.T., at given longitudes:

		h	° ′
1836	XI	14, 23	λ 137 32
1837	II	26, 1	130 9
1837	III	29, 0	128 8

A list of Mars and Jupiter at solar opposition, limited to cases that would yield by computation three planetary conjunctions in geocentric longitude for each case, is presented in the form of twenty-one entries which cover a period of two thousand years. The columns give, in order, year, month, day, hour, Julian Day, longitude of sun, and difference in time between solar oppositions of Mars and Jupiter. A recurrent cycle of 1482 years, approximately 541289 days, may be noted.

Mars.	Opp.	Hr.	G.C.T.	☉	Diff.	Jup.	Opp.	Hr.	G.C.T.	☉
22	I 7	4	1729100.17	285°.57	—2ᵈ22ʰ	22	I 4	6	1729097.25	282°.62
165	I 24	8	1781348.33	304.21	+0 16	165	I 25	0	1781349.00	304.88
308	II 11	19	1833596.79	323.01	+3 14	308	II 15	9	1833600.37	326.58
355	II 6	1	1850758.04	317.85	—4 6	355	II 1	19	1850753.79	313.60
498	II 23	22	1903006.92	336.98	—1 7	498	II 22	15	1903005.62	335.69
641	III 14	22	1955256.92	357.05	+0 18	641	III 15	16	1955257.67	357.78
784	IV 4	22	2007508.92	18.88	+1 4	784	IV 6	2	2007510.08	20.01
882	VII 18	23	2043507.96	119.70	+3 0	882	VII 21	23	2043410.96	122.58
884	X 1	21	2044213.87	193.37	+1 23	884	X 3	20	2044215.83	195.33
927	IV 30	3	2059764.12	43.58	—1 12	927	IV 28	15	2059762.62	42.13
929	VII 4	18	2060560.75	106.71	+0 20	929	VII 5	14	2060561.58	107.51
976	VI 21	8	2077714.33	94.54	—1 21	976	VI 19	11	2077712.46	92.74
1027	X 30	12	2096472.50	222.42	—1 15	1027	X 28	21	2096470.87	220.79
1170	XI 22	16	2148726.67	247.23	—1 13	1170	XI 21	3	2148725.13	245.68
1313	XII 12	23	2200977.96	269.20	+0 15	1313	XII 13	14	2200978.58	269.83
1456	XII 31	13	2253227.54	289.46	+4 4	1457	I 4	17	2253231.71	293.70
1503	XII 26	15	2270388.62	283.99	—4 1	1503	XII 22	14	2270384.58	279.87
1647	I 23	10	2322637.42	303.41	+0 4	1647	I 23	14	2322637.58	303.58
1790	II 10	17	2374885.71	322.24	+4 0	1790	II 14	17	2374889.71	326.28
1837	II 6	3	2392047.12	317.18	—4 0	1837	II 2	3	2392043.12	313.12
1980	II 25	5	2444295.21	335.77	—0 11	1980	II 24	18	2444294.75	335.31

WASHINGTON, D. C.

Planet Notes for January, 1943

By R. S. ZUG

NOTE: Greenwich Civil Time is employed unless otherwise stated. To obtain Eastern Standard Time subtract 5 hours, Central Standard Time, 6 hours, etc. The planetary phenomena are described as they are to be seen from latitude 45° N. The data are taken chiefly from *The American Ephemeris and Nautical Almanac*.

Sun. Apparent positions of the sun for January 1 and February 1, respectively, are: $\alpha = 18^h\,42^m.3$, $\delta = -23°\ 5'.6$; $\alpha = 20^h\,54^m.8$, $\delta = -17°\ 24'.3$. On January 20, the sun leaves the constellation Sagittarius and enters the constellation Capricornus.

Moon. Phenomena of the moon will occur as follows:

		h	m
New Moon	Jan. 6	12	37
First Quarter	13	7	48
Full Moon	21	10	48
Last Quarter	29	8	13
Perigee	Jan. 6	12	
Apogee	19	23	

Mercury. Mercury is an evening star, and the planet will be at greatest elongation on January 8. The declination of the planet at this time will be —16°,

so that the object will be a difficult one for observation.

Venus. Venus is now an evening star. By the end of January, the planet should be distinguishable immediately after sunset, 1¼ hours east of the sun and in declination —13°.

Mars. Mars is now inconspicuously situated in the morning sky, in declination —23°. On February 1 the planet will be $2^h\,40^m$ west of the sun. The elongation from the sun is increasing.

Jupiter. Jupiter reaches opposition on January 11. With a declination of +22°, an excellent opportunity will be afforded for the study of the planet's surface markings and satellites.

Saturn. Saturn is situated in the constellation Taurus, where it is in retrograde motion to the north of the Hyades. The rings of Saturn will be displayed at their maximum width in 1943, which fact, together with consideration of the planet's northerly declination, should give Saturn A-1 observing priority for the coming spring evenings.

Uranus. Apparent positions of Uranus for January 1 and February 1, respectively, are: $\alpha = 3^h\,56^m3$, $\delta = +20°\ 15'$; $\alpha = 3^h\,53^m9$, $\delta = +20°\ 8'.4$.

Neptune. Neptune is now situated about midway between the stars 10 Virginis and 13 Virginis.

And now, dear Readers, we say "Farewell," as, reluctantly, we relinquish this section to Miss Alice Farnsworth.

Occultation Predictions for January, 1943

(Taken from the *American Ephemeris*)

The quantities in the columns *a* and *b* are given for the purpose of making these predictions useful for any place within 200 miles of the point indicated. The procedure is as follows: Subtract the longitude of the point given from the longitude of the place in question; multiply the result in degrees, taking the signs into account, by the quantity under *a* for the star to be observed; similarly, with the latitude, using *b*; apply the sum of the products, with its proper sign, to the Greenwich C.T., and obtain the predicted Greenwich Civil Time for the phenomenon at the place of observation. To obtain Eastern Standard Time it is necessary to subtract five hours; Central Standard Time, six hours, etc.

			IMMERSION				EMERSION			
Date 1943	Star	Mag.	Greenwich C.T.	*a*	*b*	Angle E from N	Greenwich C.T.	*a*	*b*	Angle E from N
			OCCULTATIONS VISIBLE IN LONGITUDE +72° 30′, LATITUDE +42° 30′							
			h m	m	m	°	h m	m	m	°
Jan. 2	13 Lib	5.8	7 43.9	—0.2	+0.6	114	8 43.6	—0.5	+0.7	289
10	BD—7° 6036	6.4	22 29.7	—2.0	—1.0	99	23 30.3	—0.7	+1.2	208
12	33 Cet	6.2	20 16.4	—1.2	+2.0	49	21 34.1	—1.8	+0.9	256
16	70 Tau	6.4	19 56.8	+0.2	+2.2	46	20 53.3	—0.6	+1.1	280
16	θ^1 Tau	4.0	21 23.8	—0.8	+1.4	91	22 32.5	—0.8	+2.2	233
16	θ^2 Tau	3.6	21 30.6	—1.3	+0.6	116	22 22.6	—0.3	+3.2	208
16	264 B.Tau	4.8	22 38.6	—1.2	+1.8	74	23 59.9	—1.6	+1.5	249
17	275 B.Tau	6.5	0 46.5	—2.3	0.0	99	2 9.5	—1.9	+1.6	230
17	α Tau	1.1	2 28.4	—2.1	—0.4	89	3 55.5	—1.8	—0.1	249

Date 1943	Star	Mag.	IMMERSION Greenwich C.T.	IMMERSION *a*	IMMERSION *b*	IMMERSION Angle E from N	EMERSION Greenwich C.T.	EMERSION *a*	EMERSION *b*	EMERSION Angle E from N
			h m	m	m	°	h m	m	m	°
18	115 Tau	5.3	3 38.8	..	..	146	4 22.8	..	..	204
19	19 B.Gem	6.2	0 30.4	—2.0	—0.4	124	1 34.1	—1.3	+3.1	221
24	ρ Leo	3.8	9 13.5	..	..	177	9 50.4	..	..	234
25	σ Leo	4.1	9 14.6	..	..	40	9 33.3	..	..	14
30	γ Lib	4.0	10 50.7	—1.3	—0.8	135	12 3.7	—2.1	—0.1	267
	Occultations Visible in Longitude +91° 0′, Latitude +40° 0′									
Jan. 10	BD—7° 6036	6.4	21 51.8	—2.0	+0.5	78	23 9.1	—1.4	+1.0	226
16	θ^1 Tau	4.0	21 12.3	—0.1	+1.6	78	22 15.5	—0.4	+1.8	248
16	θ^2 Tau	3.6	21 15.1	—0.3	+1.1	101	22 10.9	—0.1	+2.3	225
16	264 B.Tau	4.8	22 20.3	—0.4	+2.1	59	23 29.7	—1.1	+1.3	264
17	275 B.Tau	6.5	0 9.3	—1.6	+1.4	84	1 31.4	—1.6	+1.8	239
17	α Tau	1.1	1 46.9	—2.2	+0.9	83	3 18.1	—2.1	+1.0	245
18	115 Tau	5.3	3 1.2	..	..	151	3 32.6	..	..	189
18	120 Tau	5.5	7 40.9	..	..	11	7 53.6	..	..	350
19	19 B.Gem	6.2	0 2.8	—1.0	+0.8	110	1 6.7	—0.7	+2.5	231
25	σ Leo	4.1	8 14.9	—2.4	+0.4	92	9 33.4	—1.4	—2.2	324
26	10 Vir	6.1	8 53.8	—2.5	+0.8	85	10 5.1	—1.1	—2.4	334
30	γ Lib	4.0	10 40.6	..	..	175	11 15.9	..	..	230
	Occultations Visible in Longitude +120° 0′, Latitude +36° 0′									
Jan. 12	14 Cet	5.9	5 27.9	—1.1	—3.6	127	6 1.3	—0.1	+2.8	189
16	275 B.Tau	6.5	23 40.7	—0.1	+2.0	61	0 46.6	—0.8	+1.3	263
17	α Tau	1.1	1 0.0	—0.7	+2.2	58	2 15.2	—1.6	+1.2	264
18	115 Tau	5.3	1 49.3	—1.6	+0.2	117	2 47.2	—0.6	+3.2	214
18	119 Tau	4.7	5 43.8	..	..	27	6 37.7	..	..	318
18	120 Tau	5.5	6 26.2	—2.4	+1.5	56	7 47.4	—2.0	—2.3	294
18	19 B.Gem	6.2	23 49.6	+0.2	+1.1	90	0 47.4	0.0	+1.5	251
25	σ Leo	4.1	7 27.1	—1.1	—0.4	129	8 43.7	—1.8	+0.6	276
26	10 Vir	6.1	8 6.3	—1.0	—0.3	128	9 21.8	—1.6	+0.4	282
30	190 B.Lib	6.4	14 3.7	—2.0	0.0	105	15 27.2	—2.0	—0.9	297
31	24 Sco	5.0	13 57.1	—1.8	+0.8	91	15 13.4	—1.7	—0.6	303

Asteroid Notes

By HUGH S. RICE

We have been making further observations of the minor planet Vesta, and have found that it keeps to its predicted apparent path in the sky. It is still observable in December in the evening soon after sunset, but after that, it becomes lost in the sun's rays as it comes to conjunction with the sun.

On December 3, at 0^h U.T., Vesta is 1° southwest of the star ϵ Capricorni. On December 7, it is almost identical with a 6th-magnitude star about 1¼° east and south of ϵ. On December $10^d.5$, it is 3¼° exactly south of δ Capricorni. On December 14, the asteroid is 2½° east of κ Capricorni. About December 18 it enters Aquarius. On December 24, it is 5½° southeast of μ Capricorni. An extension of a line plotted with these positions will show the place for a later time, if needed. For this month, the magnitude ranges from 7.7 to 8.0.

For 1943 we shall give the places for the four brightest asteroids as well as a few others, so that observers with small telescopes can pick up these tiny worlds without difficulty. They are all observable best during the last half of the year, but the first ones that come to opposition in 1943 can be found in the east before sunrise, long before the middle of the year.

Hayden Planetarium, American Museum of Natural History,
New York, New York, November 19, 1942.

METEORS AND METEORITES

Meteor Notes from the American Meteor Society

By CHARLES P. OLIVIER, President

FIREBALL OF 1936 JANUARY 21

A few hours after this fireball appeared, the following observation was mailed to me by Dr. P. van de Kamp, then at McCormick Observatory, University of Virginia, S10. "On January 21, 1936, at 10:25.5 P.M., E.S.T., a very bright meteor was observed at R.A. $3^h 20^m$, Decl. $-35°$. The path was irregularly downward and of a few seconds duration. Color white; brightness 10 to 100 times that of Venus; diameter about 10′; wide irregular trail that lasted a few seconds." I immediately wrote to ask for more data and on January 30 this was added by him: "I would say that $3^h 20^m$, $-35°$ is about midway between the position of the fireball when I first saw it and the end of the path. The best I can remember is that the observed path was about 5° long, was irregular downward very approximately as follows . . . (diagram given) . . . the end being only a few degrees above the horizon. The data are obviously vague but I cannot reconstruct them any better." Calculation gave for the above coordinates: azimuth 34° 21′, altitude 6° 44′. From the diagram I then deduced $a_1 = 36°$, $h_1 = 8° 44'$ and $a_2 = 33°$—, $h_2 = 4° 44'$.

As soon as the first of the above letters was received, I wrote Prof. Milton L. Braun of Catawba College, Salisbury, North Carolina, as it was obvious that the fireball must have been seen from that vicinity. He undertook a campaign of publicity, both through the papers and by personal letters. Thanks to his efforts, usable and mostly good observations were obtained from 7 persons. In several cases he met the observer and personally checked the directions given, so more confidence than usual can be placed in the data, though the number of observers is not large.

Having occasion to be away about that time, I handed the data over to a member of my staff, who was a very accurate computer. In this case, however, an error of 1 hour was made in computing the sidereal time for S10 making this key observation contradict the others. When I was shown the diagram, a solution was obviously impossible from what I saw and the papers were laid aside hoping for other observations to arrive. This did not happen, and not until very recently did I again take up the case. A little checking detected the 1 hour error, and at once the observations from S10 fitted and a solution was possible, though the fact that all the other observers were concentrated in a small area of North Carolina makes the accuracy somewhat lower than the average errors would indicate.

The color was bluish-white, and the brilliancy to persons near its path must have been comparable to that of the full Moon. The few estimates of duration differ too widely to be of value, but apparently the velocity was quite low. It did not explode nor was any long-enduring train left. Its low height at the end of the path, its low velocity, and its great brightness all are indications that fragments may have come down as meteorites. The calculated end point is nearly on the border between Union and Mecklingburg Counties and west of the village of Stout, but I do not have great confidence in the exactness of this position, as

it may well be a few miles in error. Parabolic heliocentric velocity was assumed in calculating the zenith correction.

Date	1936 Jan. 21/22 10:25.3 P.M., E.S.T.
Sidereal time at end point	91°
Began over	$\lambda = 80°\ 55'$ W; $\phi = 35°\ 44'$ N at 54 ± 6 km (4)
Ended over	$\lambda = 80°\ 43'$ W; $\phi = 35°\ 04'$ N at 17 ± 10 km (5)
Length of path	84 km
Projected length of path	75 km
Observed velocity	Probably low
Radiant (uncorrected)	$a = 166°$, $h = 25°.9$; $\alpha = 327°$; $\delta = +75°$
Zenith correction	—5°
Radiant (corrected)	$a = 166°$, $h = 20°.9$; $\alpha = 315°$; $\delta = +71°$

The bracketed figures in last column indicate the number of observations used. The radiant somewhat resembles that found for the fireball of 1934 January 11, which was recently published in these Notes. The solution was made possible by the promptness of Dr. van de Kamp in sending me his key observation, and by the energy and time put in by Dr. Braun in securing the other reports. To both I am under sincere obligation for their assistance. Similar help by scientists scattered over the United States would easily quadruple annually the number of fireballs for which paths could be computed.

Fireball of 1936 October 28

On the early evening of this date at 7:53 C.S.T., a brilliant fireball was observed over the western part of Lake Michigan. One of our very active members, Frank M. Preucil of Joliet, Illinois, was able to secure 4 observations from Joliet and one from Chicago, interviewing most or all of the people himself. H. W. Cornell of the Milwaukee Astronomical Society secured one from F. T. Vogt of that city, and the Weather Bureau sent me two clippings, one from Chicago, one from Benton Harbor, Michigan, across the lake. On the basis of these reports I attempted a solution.

Without going into needless details, the lack of any good observation either to the east or west of the path was the great handicap, because all the rest mentioned above were to the south of the path and probably not far from its vertical plane. Hence the only thing I could calculate was a fairly good end height as the data for the beginning were contradictory. The estimates of apparent diameter vary from 6′ to 20′, the magnitude from —10 to —15, the color was bluish-green, and the duration of the train about 4 seconds. It was larger at the forward end and trailed off into a sort of tapered tail. I deduce that it ended over $\lambda = 87°\ 30'$ W, $\phi = 43°\ 30'$ N, at a height of 27 ± 11 km, based upon the weighted results from 3 stations and 5 observers. If this position is correct, it would mean that any fragments fell into Lake Michigan. I am deeply indebted to F. W. Preucil and the others mentioned above for the gathering of the data which made the investigation possible.

A.M.S. No.	Date 1934	α	δ	No. →	Acc.	Notes
1677a	Jan. 19.9	216°	+ 2	5	G	
1677b	Jan. 23.9	217	+ 1	2		Confirming 1677a
1678a	Apr. 2	202	+ 5	2		
1678b	Apr. 8	212	+ 2	2		Probably confirming 1678a
1679	Apr. 21	285	+36	3	F	Lyrids
1680	July 21	305	—11	4-5	G	
1681	Aug. 19	319	— 3	4	G	

A.M.S. No.	Date 1934	α	δ	No. →	Acc.	Notes
1682a	Aug. 31	351	+42	3	G	
1682b	Sept. 1			4	..	*
1683a	Sept. 9	34	+31	3	G	
1683b	Sept. 10	33	+27	8	G	Radiant diffuse
1684	Oct. 19.9	94	+12	16	G	Orionids
1685	Nov. 2.9	153	+19	4	G	
1686	Nov. 7.9	142	+25	6-7	G	Leonids??
1687	Dec. 14.9	181	+34	4-5	F	
1688	Dec. 14.9	189	− 1	4	G	
1689	Dec. 14.9	196	+24	4-5	G	
1690	July 3.8	324	+49	6—	F	
1691	July 7.8	210	+59	5	F	
1692	July 7.8	283	+65	4	F	
1693	Aug. 6.9	42.4	+55.2	12	G	Perseids
1694	Aug. 7.9	41.8	+54.5	9	G	Perseids
1695	Aug. 10.9	43.1	+54.7	17+	G	Perseids
1696	Aug. 10.9	53.4	+57.2	5	VG	?
1697	Aug. 10.9	326.4	+66	3-5	VG	
1698	Aug. 11.9	44.4	+56.0	35	G	Perseids
1699	Aug. 12.9	27	+21	6—	G	
1700	Aug. 12.9	36.7	+54.2	10—	P	Diffuse
1701	Aug. 14.8	52	+61	5-9	P	Perseids
1702	Aug. 19.9	43	+56	4-6	F	
1703	Aug. 19.9	55	+24	4	G	
1704	Aug. 19.9	62	+56	4	VG	Perseids probably
1705	Aug. 30.6	292.5	+29	8	G	2 of the 8 on Aug. 28

*Do not give radiant themselves but conform to 1682a.

NOTE: In the above list of radiants those from 1677 to 1689 inclusive were deduced from maps submitted by Franklin W. Smith of Glenolden, Pennsylvania, those from 1690 to 1705 from maps submitted by Balfour S. Whitney of Norman, Oklahoma. The radiants were deduced by C. P. Olivier. The fraction of a day is omitted in the first list, in certain cases, because the record sheets were lent about 1934 to Hoffmeister in Germany and have never been returned, while the maps were kept here. So while in all cases the date was known from the maps, in some the hours were not available.

On the night of 1940 May 3/4 a very fine shower of Eta Aquarid meteors was observed in Florida by two groups from Stetson University, organized by Dr. Donald Faulkner, then head of the Dept. of Mathematics-Astronomy. On former occasions groups under his direction had successfully observed showers and sent in valuable data. So some of the observers had had previous experience The two stations were: S1 at Daytona Beach, Florida, $\lambda = 80° 58'.8$ W, $\phi = 28° 10'.2$ N and S2 at Altoona, Florida, $\lambda = 81° 39'.0$ W, $\phi = 28° 57'.9$ N, the coordinates being carefully determined by Faulkner from large-scale maps. The distance was 69 km, which is adequate for good results. Faulkner took charge at S2, assisted by students and there was a group of 6 students at S1. Both parties used A.M.S. maps and followed our usual rules for observing. The watches were compared for errors with respect to E.S.T. The night was good, transparency 4, which would give F from 0.9 to 1.0. At S1 observing began at 12:00, first meteor seen at 12:16, and ended at 16:35, 131 meteors having been recorded; at S2 they began at 11:30, first meteor at 11:37, and ended at 16:40, 153 meteors having been recorded.

When this fine report reached me, I thought that it would furnish the first chances to work out a large number of heights on one night for the Aquarids, a

thing I do not believe ever accomplished before. Therefore these observations have been studied and worked over far beyond what I would usually do, trying to find why more and better heights could not be deduced therefrom. In order, I did a little work myself; then the data were turned over to A. E. Hayes, one of the actual observers, who under my personal supervision did a good deal of comparing and computing; then Miss E. F. Reilly did some computing; then I turned over everything to Donald MacRae, then of our Cook Observatory staff, who put in several weeks of work; and finally I have now regretfully closed it myself, convinced nothing more can be gotten. If I write up at such length work which partly failed in its main purpose, it is not in criticism of the persons who so enthusiastically gave of their time to make the observations, but to point out the pitfalls in such work to others, so that they may have better success. In passing, I may mention that such work for heights often comes to grief, so that nearly everyone who tries it sometimes has failure. Series from elsewhere have, at times, simply had to be totally discarded.

However to explain the lack of more success in this case, I conclude as follows: An examination of the maps at S2 shows a large number of meteors whose projected paths go right through or very near the usual radiant; at S1 this is not true, only a small number satisfying the radiant. Therefore the average plotting at S1 (there were 6 observers there) was poorer than at S2, with the consequent errors in heights derived therefrom. Also examination of the times given proved that the watches in certain cases were read wrong by 1 or 2 whole minutes, the seconds being nearly correct. It was these errors that wasted so much time before we found what the correction probably was. A very ingenious graphical method by MacRae finally deduced that the time recorded at S1 was $2^m\ 26^s \pm$ faster than at S2. Next an examination of the magnitude estimates indicated that they were in general too bright. At neither station was any meteor recorded as less than 3rd magnitude. Whether all fainter ones were simply ignored is not certain, but the numbers of meteors of —1, 0, and +1 magnitudes for the number seen are too great. I also suspect a tendency on the part of some of the plotters to make their paths too long, at least the average look too long to me. It was further obvious that many coincidences were missed because the observers, at one or the other station, were looking at a given time in some direction other than the assigned one. That, and not the plotting, is why such a relatively small number of meteors were available for even attempting parallax computations. On the contrary, good points are that few errors occur in numbering paths, the arrow-head is rarely omitted, nearly every meteor recorded was plotted, and the record sheets are well filled out. These facts, along with the fine totals recorded at both stations, made me hopeful to the very end that a large number of good heights could be obtained.

The heights that are given were mostly computed by MacRae, only two by me. I fully checked a few of his results also. We used the simple graphical method, though he used the check equation for points on a great circle in perhaps a dozen cases before he computed the altitudes and azimuths. This equation is as follows:

$$\tan D_2 \sin (\alpha_2 - \alpha_1) - \tan \delta_1 \sin (\alpha_2 - A_2) + \tan \delta_2 \sin (\alpha_1 - A_2) = 0$$

(See "Meteors" p. 158.) While we do not have much confidence in the heights we deduced, the one thing neither of us can understand is the preponderance of low heights. The usual errors of observation should—and usually do—give as many abnormally high cases as abnormally low. Considerable study on the part

of both of us has not explained the matter. I do not advise the use of these heights in statistical work, and their publication is offered only that my points shall be made clear. Finally, the other valuable thing besides the radiants which can come from the many hours of work spent on these observations is to call attention to where such programs can be improved. For them to secure excellent results these things *must* be had: accurate timing at both stations, a willingness of the observers to keep their attention on the section of sky assigned to each of them, and fairly accurate plotting. If the usual A.M.S. rules are followed in addition to these, good heights should be gotten. The three radiants obtained are as follows:

A.M.S. No.	Date	α	δ	No.	Acc.	Remarks
1706	40 May 3.8	338	0	15+	V.G.	Eta Aquarids, at S2
1707	40 May 3.8	337	— 1	6+	F	Eta Aquarids, at S1
1708	40 May 3.8	257	+56	3-9	F	at S2; possibly 2 at S1

Number S2	Number S1	Time S2	ΔT S1 (m)	ΔT S2 (s)	Magn. S2	Magn. S1	Beginning S2 km	Beginning S1 km	Beginning Av. km	End S2 km	End S1 km	End Av. km	Notes
23	6	12 27 50	0	34	2	1	71	71	71	...	...	...	[1]
24	8	12 28 40	4	27	2	3	42	16	...	48	37	...	[2]
48	40	14 06 28	1	31	2	2	69	89	79	48	71	60	Sporadic
61	52	14 32 25	2	30	2	2	98	77	88	60	82	71	Sporadic
74	06	14 55 00	1	34	—2	2	92	29	...	37	63	...	[3]
82	66	15 13 08	2	32	3	2	90	74	82	81	81	81	[4]
87	67	15 21 15	2	35	1	2	87	37	62	...	...	56±	Aquarid
91	69	15 26 15	2	33	1	1	99	113	106	48	48	48	Aquarid
99	78	15 34 14	2	38	1	2	52	53	52	50	77	64	[5]
109	87	15 44 18	2	32	2	1	71	44	57	79	72	75	[6]
124	102	16 01 35	2	37	2	2	85	103	94	147	82	114	Aquarid
125	103	16 01 45	3	41	1	3	161	135	148	...	...	145±	Aquarid
126	105	16 05 47	1	37	1	1	81	58	70	106	101	104	Aquarid
130	107	16 07 20	1	42	—1	1	40	24	32	...	...	...	[7]
144	128	16 26 50	2	42	2	1	48	56	52	29	34	32	Aquarid
147	130	16 31 10	1	40	2	2	47	48	48	116	185	150	Sporadic

[1] End indeterminate Aquarid? [2] Probably not same, Aquarid? [3] Probably not same, Aquarid. [4] One observer? Aquarid. [5] One observer? Aquarid. [6] Both give yellow as color, Aquarid. [7] End indetermintae, Aquarid?

NOTE: In the above table the class of the meteors is that taken from the maps at S2, due to my belief that these plots are, on the average, more nearly accurate.

Flower Observatory of the University of Pennsylvania, Upper Darby, Pennsylvania, 1942, October 20.

Contributions of the Society for Research on Meteorites

Edited by FREDERICK C. LEONARD
Department of Astronomy, University of California, Los Angeles

President of the Society: LINCOLN LA PAZ, Department of Mathematics, Ohio State University, Columbus

Secretary of the Society: C. H. CLEMINSHAW, Griffith Observatory, Los Angeles, and Department of Astronomy, University of Southern California, Los Angeles

Nitrogen and its Compounds in Meteorites

By JOHN DAVIS BUDDHUE
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ABSTRACT

Mention is made of free nitrogen, titanium nitride (osbornite), cyanogen, and previously reported occurrences of ammonium compounds in meteorites. Ammonia is reported as present in all of the 8 meteoritic stones tested. A new method of analysis is described which was used to determine the amount of ammonia in stones of 5 falls. In Holbrook, Arizona, the ammonia is shown to be present as a compound, at least partly soluble in water. The nitrogen found by Lipman appears to have been largely ammonium nitrogen. Lipman's failure to find water-soluble nitrates or nitrites is confirmed; in addition, no cyanides were found in Holbrook.

The presence of free nitrogen in meteorites has been known since Graham found 9.86% of it in the gases obtained from the Lenarto, Sáros, Czechoslovakia, iron in 1867.[1] From the collected data of Farrington,[2] it appears that 6 stones heated to 500° C. or less contained a mean of 1.85% of nitrogen, whereas 4 irons gave 2.43% (one contained no nitrogen). At higher temperatures, the mean nitrogen-content of 11 stones was 2.10%. The mean for 9 irons was 7.83%. (One stone and 2 irons contained no nitrogen.) Dewar and Ansdell[3] found 0.1% of nitrogen in the graphite of a Xiquipilco (Toluca), Mexico, iron before digestion with ether and 0.74% afterward. The ether seems to have removed a compound which affected the other gases more than the nitrogen. Brun[4] has reported a trace of nitrogen in billitonites and also ammonium chloride, equivalent to 5 mg./kg. Moldavites contained neither. Doring and Stutzer[5] reported no nitrogen in either moldavites or Columbian glass. The writer reported cyanogen in the gases of Washougal, Washington, and Holbrook, Arizona, both stones, in 1940.[6] Osbornite was discovered by N. Storey Maskelyne in the stone of Bustee, India,[7] but its chemical composition remained uncertain until Banister[8] showed that it is in all probability titanium nitride, TiN.

When Dewar and Ansdell[3] heated an Orgueil, France, stone they observed a sublimate which they said was composed mainly of ammonium sulfate, with traces of sulfides and sulfites. A considerable quantity of water also was obtained and this contained either ammonia or its salts or both. Lipman[9] extracted Forest City, Iowa; Gilgoin No. 2, New South Wales, Australia; and Holbrook, Arizona, stones with water. The extract was tested for ammonia with Nessler's solution, but the result was negative. Nitrite and nitrate also were absent. I have extracted Holbrook for 3 hours with ammonia-free water in a Soxhlet extractor and obtained a strong positive ammonia reaction with Nessler's solution. I confirm the absence of nitrate and nitrite. Aerolites from Morland, Kansas; Alamogordo, New Mexico; Kimble Co., Texas; Arapaho, Colorado; Holbrook, Arizona; Covert, Kansas; Ness City (Ness Co.), Kansas; and La Lande, New Mexico, all were found to contain more or less ammonia. In these cases the test consisted of fusing the powder with caustic soda and mercurous nitrate. The evolved gases were swept out of the tube and through Nessler's solution by a current of air. Blanks showed that the air and reagents were ammonia-free. The mercurous nitrate was used to prevent the alkali from forming ammonia from any cyanides that might be present.

In order to determine accurately the amount of ammonia present in some of these stones, a new method of analysis was devised. A weighed sample of meteorite was treated with mercurous-nitrate solution, dried, and then heated to redness with an excess of caustic soda, in a hard-glass test-tube. A slow current

of air was passed through the tube and bubbled through a solution of silver nitrate and formaldehyde. The precipitated silver was washed, dissolved in nitric acid, and determined in the usual way, except that sometimes the iodide was used in place of the chloride. The reaction involved is:

$$2AgNO_3 + 6NH_4OH + HCHO = 2Ag + HCOONH_4 + 2NH_4NO_3 + 3NH_4OH + 2H_2O.$$

Theoretically, 3 moles of ammonia produce one mole of silver and leave 1.5 moles of ammonia unused. This unused ammonia releases a further 0.5 mole of silver and so on, so that 3 moles of ammonia would be expected to liberate $1 + \frac{1}{2} + \frac{1}{4} + \ldots + \frac{1}{2^{n-1}}$ moles of silver. The limit of this series is 2, so that the ratio of ammonia to silver is 3:2 or 1:1.5. Experiment shows, however, that this ratio is not realized, because, when the concentration of ammonia falls below a certain value, no silver is liberated. The experimentally determined gravimetric factor for converting silver weights to ammonia weights under the conditions of the analysis turns out to be 0.1396. The advantages of this method of analysis are its simplicity and the fact that the weight of silver produced, especially when determined as iodide, is so much greater than the weight of ammonia that small errors in weighing *etc.* do not greatly affect the amount of ammonia found. Representatives of 5 aerolitic falls were analyzed for ammonia by this method with the following results:

Meteorite	% of Ammonia	Remarks
La Lande, New Mexico	0.0023	Contained some organic matter.
Ness City, Kansas	0.0013_5	
Covert, Kansas	0.0006	
Morland, Kansas	0.0020	
Holbrook, Arizona	0.0033	With mercurous nitrate.
Holbrook, Arizona	0.0030	Without mercurous nitrate.
Holbrook, Arizona	0.0011_5	After Soxhlet extraction.

Of the 2 unextracted samples of Holbrook, the result for the first listed is the more accurate, as it was found that a very small part of the second had escaped the action of the alkali. The agreement of the results with and without mercurous nitrate and the fact that only a doubtful trace of cyanides could be found in Holbrook indicate that cyanides are not present in these stones, altho there is spectroscopic evidence of the presence of cyanogen itself[6]. The ammonia in Holbrook is not present in the free state, as not any could be detected in the gases given off when a sample of the powdered meteorite was heated up to the temperature of the softening point of glass. Since the extracted sample of Holbrook contained only 23.% of the original ammonia content, it follows that at least 65% of the ammonia compound or compounds are soluble in water.

My determination of the ammonia in Holbrook is equivalent to 0.0027% of N. Lipman,[10] using the Micro-Kjeldahl method, found 0.0025%, and 0.0016% of N in 2 specimens of this fall. My result is in good agreement with one of his, a fact that seems to indicate that the bulk of the nitrogen he found was ammonia nitrogen. His other result is so far below all the rest of his determinations that an error is indicated, or else the sample used had previously been subjected to the action of water. The particular ammonium salt or salts involved are not known, but my previous work on the soluble constituents of meteorites[11] excludes the chloride, in the case of Holbrook.

My thanks are due to the Plastics Institute, 186 S. Alvarado St., Los Angeles, California, for the use of their laboratory facilities.

Addendum: Percy E. Spielmann[12] alludes vaguely to "basic nitrogen" in meteorites and suggests that it is derived from the action of water on a nitride, but he gives no reference.

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The Enon, Ohio, Meteorite (Mesosiderite)

By H. H. NININGER
Colorado Museum of Natural History, Denver

ABSTRACT

This paper tells of the recognition and the recovery of a small (763-g.) mesosiderite, known as the Enon, Ohio, meteorite; it contains also some interesting notes on both the extent and the range in composition of meteorites.

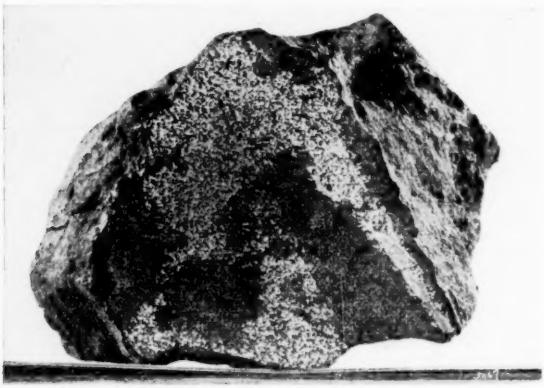
The small, but unique, Enon, Ohio, meteorite narrowly escaped being lost to science. It was reported to the American Meteorite Laboratory through the coöperation of Mr. Frank Clay Cross, to whom a very small sample was sent, in 1918, by its finder and long-time possessor, Mr. U. G. Drummond of Elizabeth, Colorado.

The writer, accompanied by Mr. Cross, drove out to the home of Mr. Drummond, after examining the small sample. Both of us rejoiced in the thought that we were to have the privilege of adding a new name to the list of Colorado meteorites and wanted to check up on the facts of its discovery. To our surprise, we learned that the specimen was not a Colorado find at all, but that it had been picked up in Ohio many years ago, about 1878! It had not been recognized as a meteorite until literature on how to recognize meteorites had reached its finder a few weeks previous to our visit. Our description had aroused suspicion, and the sample had been forwarded to settle the question.

It seemed strangely fortunate that, for 60 years, this homely little stone had been sufficiently cherished by its finder to escape being lost, while he resided successively in Iowa, Colorado, Wyoming, Utah, and then at 2 more locations in Colorado. When asked why he had so jealously clung to such a shapeless, rusty-looking object, he replied that, because of its weight, he had kept it as a souvenir of his father's farm! In the light of this unusual record, we wondered how many meteorites had been picked up and later discarded by persons who had been less sentimental regarding their old homesteads!

The specimen plainly showed an abundance of metal, on grinding a small corner. It fell, apparently, into the class of mesosiderites. Its total weight was only 763 g., but it looked so intriguing that a section was removed and the cut surfaces were polished. This treatment revealed a very unusual structure, the

pattern of which was similar to that of other mesosiderites, but, in the Enon specimen, about half of the "metallic grains" were not true metal, but sulfide. The accompanying photograph shows well the relative abundance of the metal and the sulfide. In both the sulfide and the nickel-iron areas, the very irregular



A Polished Section of the Enon, Ohio, Mesosiderite

grains frequently joined to form a reticulum, and, in certain places, the reticulum merged into single veins. One of these veins is well shown in the lower, right-hand corner of the photograph. The vein consists of about equal parts of metal and sulfide.

This meteorite is very important in that it suggests the probable existence of meteorites in which all of the metal is lacking and, in its place, sulfide forms the reticulum. A few pallasites suggest the same condition. The writer has found considerable areas in the Springwater, Saskatchewan, Canada, pallasite where only sulfide separated the olivines. Similar areas have been noted in the Admire, Kansas, and Albin, Wyoming, pallasites. Certain stony meteorites of the chondritic class have shown a preponderance of sulfide grains over nickel-iron grains. This fact is strikingly true of the Potter, Nebraska, meteorite, of the New Almelo, Kansas, aerolite, and of the Kelly, Colorado, stone. The sulfide in meteorites has been found to be usually pyrrhotite.

O. Sjöström found on careful analysis of pyrrhotite from the Cranbourne, Victoria, Australia, meteorite (siderite) that it carried 4.34% of nickel and 1.52% of cobalt. (The pyrrhotite of most meteorites carries only a fraction of 1% of nickel.) These metals apparently substitute for a portion of the iron in Cranbourne pyrrhotite, which was only 58.07%, instead of the usual 62% to 64%. If we may assume the existence of large meteorites of planetoidal dimensions, in which pyrrhotite of the forenamed composition is a prominent constituent, then we have a plausible explanation of such nickel deposits as that at Sudbury, Ontario, Canada. The saucer-like deposit at Sudbury, 16 by 36 miles, conforms very well in outline to the small Haviland, Kiowa Co., Kansas, meteorite crater, which represents evidently a low-angle impact of a mass of only a few tons. A mass of many millions of tons, practically unretarded by the atmosphere, would depress and fracture the crustal rocks and would form a large pool of fused meteoritic and

terrestrial magma, whose settling might account for the arrangement of the ores which have puzzled Sudbury geologists, working on orthodox assumptions.

Those of us who have spent years in searching for witnessed and unwitnessed meteoritic falls have, year by year, been forced to broaden our concepts of both the extent and the range in composition of meteorites. When a witnessed fall has been finally run down and collected, we are quite as likely to find that it belongs to one of the so-called rare varieties, such as the howardites, chladnites, eukrites, nahklites, or carbonaceous forms, as to a type that is better known. Obviously, those meteorites that more closely resemble terrestrial rocks are never distinguished from them except under unusual circumstances, such as an exhaustive search following a spectacular fall. We should like to make it clear, however, that this deduction is based upon those recoveries concerning the meteoritic character of which there can be no doubt, in the minds of even those who still cling to the orthodox criteria of identification. Concerning problematical meteorites, lying outside the recognized limits of classification, we shall have something to say in a separate paper.

The meteorite herein described shall be known as the Enon, Ohio, meteorite (mesosiderite). Enon is situated in Clark County, between the cities of Springfield and Dayton.

"A Comet Strikes the Earth" (Review)

"A Comet Strikes the Earth" is the title of a popular booklet of 40 pp. and 8 ill., by H. H. Nininger, describing the Canyon Diablo, Arizona, Meteorite Crater and explaining how to recognize meteorites. A feature of the pamphlet is an attached fragment of "iron shale" (oxidized meteoritic material), which the author calls "a genuine meteorite," collected by him within a distance of 2 miles of the crest of the Crater rim. The booklet sells for 35 cents a copy, or $1.00 for 3 copies, postpaid by the American Meteorite Laboratory, 635 Fillmore St., Denver, Colorado.—F.C.L.

"The Portland Meteor and Resulting Meteorite" (Review)

"The Portland Meteor and Resulting Meteorite" is the title of *Astron. Soc. of the Pacific Leaflet No. 165*, Nov., 1942 (pp., 1 f.), by J. Hugh Pruett of the University of Oregon, the Pacific Director of the American Meteor Society and a Councilor of this Society. The article contains a highly interesting, popular account of the great "Portland meteor" of 1939 July 2, and of the Washougal, Washington, aerolite, which fell from it and for whose recovery Professor Pruett himself was mainly responsible (*v. C.S.R.M.*, **2**, 138-42, 1938-41; *P. A.*, **47**, 500-4, 1939).—F.C.L.

Comet Notes

By G. VAN BIESBROECK

Comet 1942 *e* (Oterma). A cablegram from Copenhagen received here November 10 announced the discovery of a new comet by Miss Oterma at the Turku (Abo) Observatory in Finland. The first information received gave the following information:

1942 Nov. 6 at $20^h\,16^m$ U.T. Magnitude 13.
Right ascension $4^h\,11^m.0$
Declination $+0°\,47'$
Slow motion northward.

The comet was readily picked up here last night. It appeared appreciably brighter than the discoverer announced. By comparing it extrafocally with neighboring stars I estimated the total brightness as 10.6 mag. in the 24-inch reflector. The comet showed a sharp nucleus surrounded by a coma of 3′ diameter, nearly round although the brighter south-east side seems to indicate a vague indication

COMET 1942 *e* (OTERMA)
1942 NOVEMBER 11

of tail in that direction as seen in the Figure, enlarged from a 20-minute exposure at the 24-inch reflector.

The position measured by the writer

1942 Nov. 11 at $4^h\ 22^m.7$ U.T.
Right ascension $4^h\ 10^m\ 21^s.29$
Declination $+2°\ 0'\ 47''.8$

confirms the northward motion of about 17′ daily, while the right ascension decreases only 9ˢ daily. It is too early to predict anything about the future course of this object.

COMET 1942 *b* (OTERMA). The same Turku observer whose discovery was just reported was responsible for an earlier comet discovery this year, news about which has reached this country only late in October. The object, described as diffuse, without condensation and of magnitude fifteen, was observed there several times beginning February 11 and from these data the discoverer herself computed the following preliminary parabolic orbit:

Perihelion date	1942 Sept. 7.18 U.T.
Longitude of perihelion	159° 20′
Node	279 44
Inclination	172 34
Perihelion distance	4.2068

Dr. Whipple of the Harvard College Observatory has computed a rough ephemeris from which it appears that this distant object should still be in reach of large instruments. However, the position is quite uncertain after such a long interval.

Of the previously announced comets only COMET SCHWASSMANN-WACHMANN No. 1 remains under observation. It has again undergone large fluctuations in

shape and brightness. Last night I recorded it a very diffuse coma of magnitude 15.

The search for COMET 1928 I (REINMUTH), expected in the morning sky, has not yet led to its recovery. But that object is much better placed in the beginning of next year or so it will probably be located before long.

Williams Bay, Wisconsin, November 11, 1942.

VARIABLE STARS

Variable Star Notes from the American Association of Variable Star Observers

By LEON CAMPBELL, Recorder

Harvard Bulletin on Variable Stars: The *Harvard Bulletin* dated October 1, 1942, contains fourteen separate papers on variable stars, some of which have already been referred to in these notes. Attention is now called to the other interesting items.

TX Ursae Majoris, a well-known eclipsing variable, has been studied by Dr. Cecilia Payne-Gaposchkin using the photographic measures made by Mr. Huruhata and Dr. O'Keefe. The definite change in period is confirmed, and may be represented by a sine curve with an amplitude of $0^{P}.016$ with a period of 10,000 to 12,000 days. A photoelectric light curve by Dr. A. A. Wood reveals a displacement of the secondary minimum with respect to the primary minimum, indicating an apsidal rotation in a period of about thirty years.

Variables in N.G.C. 4833: There are now known to be eleven variables in the globular cluster N.G.C. 4833. All of these variables have been measured recently by Miss Frances W. Wright on Harvard plates, and of these eleven, there are seven of the cluster type with periods less than $0^{d}.75$—average of six, $0^{d}.667$, two are Cepheids—one with a period of $87^{d}.7$, and two are long-periods with periods of $333^{d}.7$ and $303^{d}.8$, respectively.

The mean median magnitude of the cluster variables is 15.65 pg., which accordingly gives a distance for the cluster, uncorrected for absorption, of the order of 13,500 parsecs.

Variable Stars in M.W.F. 377: Dr. S. L. Thorndike has studied M.W.F. 377—situated at 21^{h}, $+5°$—with the result that of the 29 variables there investigated, twelve are of short period, six are cluster Cepheids, six are long-periods, two are eclipsing variables, one is semi-regular, one is a classical Cepheid, and one is unclassified.

Prediscovery Observations of SS Cygni: Photographic observations of SS Cygni obtained before its discovery by Miss Wells in 1896 have been collated by Miss H. H. Swope. Every plate on which the star appears was measured, and they date from November 6, 1889, when the star was at one of its maxima, to November, 1896. The variable was observed on 107 plates, but the observations are too scattered for one to derive more than a few maximum dates. These indicate that from 1889 to 1896 a mean interval of 41 days between maxima is most common; this value is in striking contrast to a mean period of 50.4 days found from the 40-year discussion of the visual observations. The photographic range

in magnitude appears to exceed the visual range, the star being brighter at maximum and fainter at minimum.

Secondary Light Variations in α Herculis: Joseph Ashbrook finds that a secondary variation exists in the red variable α Herculis. The light curve between 1932 and 1941 is characterized by a succession of irregular waves of amplitudes $0^M.3$ to $1^M.0$, and the intervals between consecutive maxima are between 54 and 122 days.

The Eclipsing Binary, 66.1939 Cygni: Mr. Ashbrook has also studied the light variations of 66.1939 Cygni, an eclipsing variable star originally discovered by Wachmann. Ashbrook finds that the star is of the Beta Lyrae type with a period of $6^d.52$, and varies between photographic magnitude 7.94 at maximum, and $8^M.47$ and $8^M.25$, at principal and secondary minimum, respectively. There appears to be no evidence of a change in period over the last 40 years. Elements have also been derived which give radii of the two components as 0.361 and 0.170, luminosities as 0.692 and 0.398, ellipticity as 0.226, inclination as 79°.5, and masses as 0.0039 ⊙ and 0.027 ⊙.

New Bright Cepheid Variable in Scorpius: Another paper by Mr. Ashbrook describes a new bright Cepheid in Scorpius, C.P.D. —45°8503. He derives a period of 6.7963 days, amplitude of variation from $7^M.46$ to $8^M.06$ pg., and a light curve closely resembling that of U Sagittarii, period 6.74 days.

The Peculiar Variable SW Ceti: Dr. R. Prager has been studying the light variations of SW Ceti. He has examined 1011 Harvard photographic observations extending from 1889 to 1941, and finds that the star does not belong to the long-period class, and that cycles of about 400 days do not occur, as had been previously announced by other observers. By forming mean brightnesses for every season, it appears that these seasonal means generally vary little and keep near to the lowest level. But three maxima are indicated in 1911, 1928, and 1941. The maximum of 1928, which is the highest, develops after a slow, continuous increase of the brightness through ten years. The spectrum has been classed as M4 in 1923 from Harvard plates, and as M8 by Vyssotsky at the McCormick Observatory. The star can certainly be described as peculiar.

Two Photographically Observed Minima of ε Aurigae: Dr. S. Gaposchkin has made photographic observations of two minima of the long-period eclipsing variable ε Aurigae and finds the photographic range to be between $3^M.73$ and $4^M.53$, or $0^M.80$. Visual ranges were found to be $0^M.76$ and $0^M.71$ for 1875 and 1901, respectively, while the photoelectric range in 1928 was $0^M.80$, which agrees with the photographic range in 1901 and 1928.

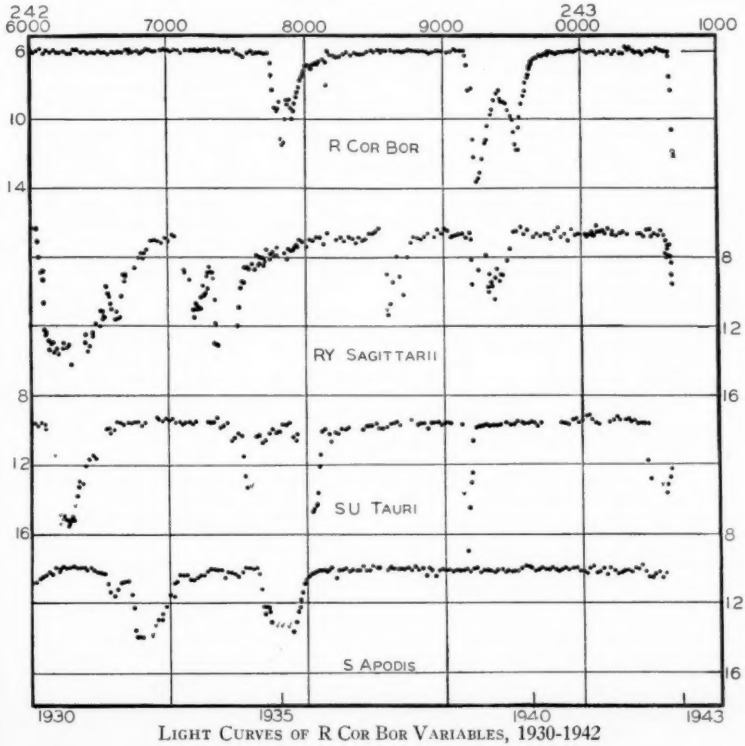
Ten Spectroscopic Binaries: Another study by Dr. S. Gaposchkin deals with an attempt to detect possible light variation in ten spectroscopic binaries for which there has been no previous evidence of variability. Although spectral periods had been found for these ten stars, ranging from 2.5 days to 7.35 years, none of them shows marked evidence of variability with the spectral periods assigned.

Proper Motions of 24 Eclipsing Systems: Dr. Z. Kopal has derived proper motions for 24 eclipsing systems and lists their annual proper motions in Right Ascension and Declination, together with their probable errors. These proper motion values range in R.A. from —0".062 to +0.080, and in Dec. from —0".70 to +0".045. The range in spectral class includes B1 to G6.

Eclipsing Variables in the Magellanic Clouds: Dr. Shapley and Miss McKib-

ben report on a study of eclipsing variables in the Magellanic Clouds. It is well known that highly luminous eclipsing variables are infrequent, whatever their spectral class, and especially uncommon are those with periods between one and ten days. Sixteen eclipsing variables in the Clouds, out of a total of nearly 700 variables of all types, are listed by the authors with periods between 1.25 and 66.93 days. Mean photographic maximum magnitude is 15.27, for primary minimum 16.08, and for secondary minimum, 15.59. Six variables are classed as probably belonging to the Algol type, and ten as of the Beta Lyrae type. It is assumed that these eclipsing variables are superposed upon the Clouds, otherwise their absolute magnitudes at maximum, —1.4 to —3.2, would be exceedingly high for such stars with average period length and typical light curves. Nine of these sixteen variables were found in the Small Cloud and the others in the Large Cloud.

The R Coronae Borealis Stars: Reference was made in last month's "Notes" to the remarkable coincidence found in the times of decadence in light of three of the four well-observed R Coronae Borealis variables. In recent months RY Sagittarii and R Coronae Borealis began their descent at approximately the same time, while SU Tauri was then apparently on the rise from a narrow minimum,



LIGHT CURVES OF R COR BOR VARIABLES, 1930-1942

having faded away to below magnitude 13.6 during July of this year. A similar coincidence appears to have occurred in 1938-39 (see these notes for November, 1939) and also during 1935, when R Coronae Borealis, SU Tauri, and S Apodis were faint. In 1931 RY Sagittarii and SU Tauri were faint at the same time. S Apodis has been at normal maximum brightness, around magnitude 10, since 1936, with only slight indications of variability.

While it is not likely that any sort of periodicity exists for this type of variable, it is to be noted that, during the past dozen years, intervals of from 900 to 1300 days have prevailed for all four stars. It should perhaps be emphasized that the coincidence of minima is pointed out without intending to suggest a tie-up between these stars.

New Nova in Puppis: Word has just been received from La Plata of the discovery by Professor B. H. Dawson of a bright nova in Puppis, of nearly the first magnitude. It is located in R.A. $8^h\ 9^m5$, and Dec. $-35°\ 12'$. Although the star is rather far south for northern observers, yet it should be observable near the meridian. This is the only naked-eye nova that has appeared in the sky since the discovery of Nova Lacertae in 1936, and it was exceeded in brilliance by Nova Aquilae of 1918. Observations made on November 11 and 12 indicate a slight increase in brightness, at magnitude 0.9 and 0.6, respectively. Reports of estimates of magnitude contributed to the A.A.V.S.O. at Harvard Observatory, Cambridge, Massachusetts, will be most welcome, especially from observers in the southern hemisphere.

Observations received during October, 1942:

Name	Var.	Obs.	Name	Var.	Obs.
Ashbrook	2	5	Hudlow	5	5
Ball, A. R.	24	24	Jones	54	145
Blunck	32	45	Kearons, Mrs.	80	132
Boone	3	3	Kelly	12	13
Bouton	42	68	Koons	85	144
Buckstaff	15	35	Leavitt	6	6
Campbell	1	1	Luft	10	75
Cousins	49	98	Manlin	55	55
Dafter	24	60	Maupomé	73	73
deKock	115	352	Meek	28	148
Duffie	26	44	Nadeau	35	45
Erdman	12	18	Parker	64	79
Fernald	222	417	Parks	26	46
Garneau	24	32	Peltier	171	263
Griffin	69	70	Reeves	2	4
Halbach	2	2	Rosebrugh	20	146
Hale	4	5	Schoenke	30	62
Harris, H. M.	59	60	Segers	9	16
Harris, R. G.	4	4	Sill	76	76
Hart	24	24	Topham	38	38
Hartmann	162	206	Vohman	25	31
Heckencamp	5	7	Webb	28	35
Hiett	14	22	Weber	93	100
Holt	125	243	—		—
Howarth	16	16	48	Totals	3598

November 13, 1942.

General Notes

The beginning of Volume Fifty-one is an event which is deemed worthy of recognition. We are planning to incorporate in the next issue some features which will be commemorative of that event. POPULAR ASTRONOMY should be given an auspicious start on its second half-century.

Dr. Richard S. Zug, now engaged in war work at the Ballistic Research Laboratory at Aberdeen, Maryland, because of lack of time finds its necessary to discontinue the preparation of Planet Notes. His services in this particular in the past have been greatly appreciated. However, we accept the situation and shall relieve him from this responsibility.

Beginning with the next issue, Planet Notes will be regularly contributed by Dr. Alice H. Farnsworth, of Mount Holyoke College.

The Rittenhouse Astronomical Society held its monthly meeting on Friday, November 13, in the Lecture Hall of the Franklin Institute in Philadelphia. Dr. H. R. Morgan, Principal Astronomer, United States Naval Observatory, gave an address on the subject, "The Motions of the Earth and of other objects in the Solar System." Future meetings are announced for December 11, January 8, February 12, and March 13.

Time Signals from Mexico

Beginning with the first day of November of this year the radio station X.E.T.T. Mexico is broadcasting a time signal every minute, originating at the National Astronomical Observatory of Tacubaya, Mexico City. One of the clocks of the Observatory makes an electric contact every minute and the current acts upon a bell of the station. A clerk states the hour and minute corresponding to the signal. The frequency of X.E.T.T. is 9555 kc. and the signals can be heard in a great part of the American Continent. Any report about the signals will be duly appreciated.

DR. JOAQUIN GALLO, *Director.*

Tacubaya, 14 November 1942.

Nova Puppis 1942

According to *Harvard Announcement Cards* 638 and 639, the star which is given the above designation was observed by Dr. W. W. Morgan at Yerkes Observatory and by Dr. A. D. Maxwell at Ann Arbor on the morning of November 11, its magnitude at that time being estimated at 0.5 to 1.0. On November 12, Mr. Leon Campbell of the Harvard College Observatory determined the visual magnitude to be 0.6.

A letter from Mr. H. B. Rumrill, Berwyn, Pa., states that he noticed a bright star in the position of the nova on the morning of November 9, but, not thinking about novae, failed to sense the significance of his observation.

A letter from Mr. Earle B. Stradley, 1226 16th St. N.W., Washington, D. C., gives the information that he too independently noticed this bright star on the

morning of November 12 and again on November 14, clouds making observations impossible on November 13. Mr. Stradley's estimate is that on November 12 the magnitude was in the neighborhood of 0.7 and that on November 14 it had declined to about 1.2.

It seems, therefore, that this nova has had several discoverers.

First Determination of the Velocity of Light

A new series of publications devoted to outstanding scientific discoveries has been auspiciously launched by the Burndy Library, with a brief study that is important for the history of physics, astronomy, and optics. The subject is Ole Roemer's refutation almost three centuries ago of the traditional doctrine of the instantaneous propagation of light and his determination of its finite velocity. The author of the little work is a member of the Physics Department at Harvard University, I. Bernard Cohen, whose recent edition of Benjamin Franklin's experiments on electricity has won high praise. His analysis of Roemer's achievements had the misfortune to fall into the hands of the Nazis when they overran Belgium in those dark days of 1940, for it was published in *ISIS*, a learned journal which was formerly printed in Belgium. When that unhappy little country was battered into submission, Cohen's study of Roemer became, for all practical purposes, unavailable to the English-speaking world. Because of the interest which it has aroused, the Burndy Library has now decided to re-issue it.

The young Dane, Ole Roemer, utilized cosmic spaces and astronomical measurements to determine the velocity of light. In his work on the satellites of Jupiter he had noticed that immersions of the first satellite recurred in a shorter mean period than emersions. He explained the difference between the periods as due to the time required by the passage of light. On this basis he predicted in 1676 that the first satellite would be eclipsed ten minutes later than expected. The event bore out his forecast. Thus Roemer replaced the long-established notion of instantaneity by the correct view that light has a finite velocity. Although his determination of that velocity was too low and had to be revised, it was of the right order of magnitude.

A useful bibliography and index brings this little volume to a close. The Burndy Library is to be complimented on the attractive format in which it has presented its first offering. It is to be hoped that the same high physical and intellectual level will be maintained in its forthcoming publications, Galvani's *Electrical Force in Muscular Movement*, Kepler's *New Star*, and Copernicus's *Revolutions*.

Copies available from Burndy Library, Inc., 107 Eastern Blvd., New York City 50 cents per copy.